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**Process of argument: a contribution to l**



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“ Je pris garde que, pour la logique, ses syllogismes et la plupart de ses autres instructions servent plutôt à expliquer à autrui les choses qu'on sait, ou même, comme l'art de Lulle, à parler sans jugement de celles qu'on ignore, qu'à les apprendre ; et bien qu'elle contienne, en effet, beaucoup de préceptes très-vrais et très-bons, il y en a toutefois tant d'autres mêlés parmi qui sont ou nuisibles ou superflus, qu'il est presque aussi malaisé de les en séparer que de tirer une Diane ou une Minerve hors d'un bloc de marbre qui n'est point encore ébauché.”—**DESCARTES.**

“ Verus experientiae ordo primo lumen accendit, deinde per lumen iter demonstrat, incipiendo ab experientia ordinata et digesta et minime praepostera aut erratica, atque ex ea educendo axiomata, atque ex axiomatibus constitutis rursus experimenta nova.”—**BACON.**

THE  
PROCESS OF ARGUMENT

A CONTRIBUTION TO LOGIC

BY

ALFRED SIDGWICK

AUTHOR OF

"FALLACIES," "DISTINCTION, AND THE CRITICISM OF BELIEFS," ETC.

LONDON

ADAM AND CHARLES BLACK

1893

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## PREFACE

THIS book, like my former ones, has for its aim the extension of a knowledge of the more useful parts of Logic. It is written for those who are interested rather in the war against fallacy than in the grammatical inquiries which form so large a part of the Logic taught in the text-books.

Some care has therefore been taken to use words as far as possible in their everyday sense. Wherever it has seemed more convenient to depart at all from the commonest custom, reasons are given and the departure is left optional. No attempt is made to force the reader to accept hard doctrines or strange definitions, which are not yet his own.

It is specially in regard to the meaning of technical terms that this negative mode of treatment shows itself. In Logic, as in other subjects, the leading terms are capable of better and worse definition, and it is not unusual to find that doctors differ on the question which definitions are best. In all such cases the aim of this book is to help the beginner to improve his own first notions for himself, rather than to get him to accept any ready-made ones which happen to seem satisfactory to some particular school of thought.

Appendix A is part of an article contributed to *Mind*, which the Editor kindly allows me to reprint. Special thanks are due to Mr. Carveth Read for the many improvements he has helped me to make throughout.

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## CHAPTER I

### INTRODUCTORY

IN the following inquiry we shall purposely avoid all direct search for the best definition of the word "Logic." Though this book is certainly meant as a contribution to Logic, it is not meant to contribute to it in that particular way. Interesting though such a question may be, its importance is of a very special and limited kind; and at any rate there is no need to wait for a perfectly comprehensive and final idea of the limits of a subject in order to make a beginning with it. Knowledge of Logic, like other knowledge, may be acquired by degrees.

Some general guiding idea, however, will

probably be of service, for the sake of putting our views together and regarding them as means to an end. For this purpose the broadest and commonest notion of Logic is quite sufficient. Let us say, for instance, that Logic is all about arguments, and helps us to distinguish sound arguments from unsound ones ; or that Logic attempts to supervise our methods of judging, or of convincing ourselves that a given assertion is true or false. No one can quarrel with these loose definitions except on the ground of their incompleteness, and to us their possible incompleteness does not matter. The process of argument is just what is here to be discussed, and especially with a view to the distinction of sound argument from unsound. We are to inquire in what way any reasoned belief lies open to attack ; and so to survey objections generally,—the objections that can be brought against the truth of any disputable belief, or “judgment,” or assertion.<sup>1</sup>

<sup>1</sup> The words “belief,” “judgment,” and “assertion” may here

The process of forming judgments is, for human beings, almost inextricably entangled with the process of criticising the judgments as they are formed. The question whether the lower animals "judge" at all depends on what we choose to mean by the word. It is plain that many of them distinguish persons, and foods, and recognise signs of danger, or signs of what they are seeking. But at any rate they judge less deliberately and critically than we do; so that, to speak broadly, they do not stop to weigh their judgments or to reflect upon the likelihood of error. And apart from any other and deeper difference between ourselves and the beasts, it is the process of conscious reflection upon our judgments that chiefly makes our mental operations distinct from theirs. For good or ill, our judgments are on the whole more

be taken as synonymous. There would be no harm in carefully making differences of meaning between them, but we are not here concerned to do so. Our plan, as already said, is to adopt the wide ordinary usage of words wherever we are not actually driven to depart from it.

guarded, less impulsive, more conscious of the victory over doubt.

This weighing of judgment, this balancing of reasons for and against the truth of a belief, this awakens of our critical faculty, is here to be included under the notion of "argument." We shall not restrict the word argument to mean only disputation between two parties, but shall take it in the widest possible sense. At any rate the process which is here to be discussed occurs in the mind of an individual, as well as where two individuals are disputing; and we may call it the process of argument in default of a better name. So understood, there is argument wherever an inference is critically drawn, or wherever a judgment is critically formed, even when the criticism against which it stands firm proceeds from our own critical faculty in the absence of any opponent. Still, the process can best be observed in cases where there is a conflict of opinion, and therefore there is some convenience in draw-



ing illustrations chiefly from argument in the narrower sense.

Objections against an assertion, on the score of its truth, may be divided under two heads,—the objection that it is based upon false “facts,” and the objection that it involves a false inference from facts that perhaps are true. For a reason which is explained in the next chapter, it is enough for Logic to concern itself with the latter form of objection only.

Our plan therefore will be, first to discuss the nature of *Inference* generally,—meaning by inference the *reading of signs*; that is to say, the inferring of one supposed fact from another or others. There is also a different process of “Inference” usually recognised in books on Logic, namely, that of reaching a conclusion by means of merely verbal transformations. From a given *sentence*—say, “All men are mortal”—we may infer certain other *sentences* to be true; for in-

stance, that "No immortals are men," or that "Some men are mortal," "Some mortals are men," and so on. Or, again, we may often take two sentences, and by leaving out a part of both of them, arrive at another sentence (called the "conclusion") whose truth is involved in theirs. To take again the hackneyed example, we may draw from the two sentences "all men are mortal," and "Socrates is a man," the one sentence "Socrates is mortal." There is room for much ingenuity in following out the laws of this kind of inference, and in speculating on the grammatical and other questions suggested by the study of them. But this function is admirably performed already by a host of books too numerous to mention, and too complete to leave room for much improvement. We shall therefore here turn our backs resolutely upon all inquiries into the proper meaning of forms of sentence, singly or combined, and assume in the reader just that knowledge of ordinary grammar which

the general reader is likely to have. Some error does, no doubt, arise in practice from our occasional failure to see equivalent meanings in different forms of words, but the chief source of actual errors in judgment certainly lies elsewhere, — namely, in our limited acquaintance with the facts and the laws of Nature. We infer one “fact” wrongly from another, for the most part not through failing to understand the full meaning of a sentence, or tangle of sentences, but through failing to understand the full meaning of what we observe, or take as true. We look at the surface of facts and judge of their nature hastily ; we leap to conclusions without testing them sufficiently ; we generalise on scanty data. The question then arises, What can be done towards combating errors in our interpretation, not of sentences, but of facts that are taken as true ?

So comprehensive a question admits of various treatment, according as we make it more or less elementary. The line of treat-

ment here adopted is as follows: inference, we find, always involves generalisation. That is to say, we never leap from observed fact to unobserved, except by the help of our knowledge of Nature generally. If the sight or smell of smoke leads us to infer the presence of fire, that is because we believe fire and smoke to be causally connected; if the falling barometer leads us to infer the coming storm, that is because we believe there is some general rule, loose though it be, as to their concomitance. In short, to connect any two facts as sign and signification, is to believe that they are connected by natural law. Inference is only an aspect of generalisation, though no express and conscious generalisation may, in a given case, be formulated. In the entire absence of supposed general rules, any fact would be wholly meaningless to us; it would be strictly *sui generis*, and therefore unintelligible or monstrous. Thus an apparition is feared because we cannot explain it or foresee what will happen next.

Since, then, our inferences from fact to fact depend upon our belief in *general rules* of connection between fact and fact,—generalisations about the way things happen in Nature,—the work of criticising inferences resolves itself into that of criticising generalisations. Of any given inference we have first to ask what generalisations underlie it, and then what can be done towards establishing these as true.

The former aim—the discovery of the underlying theories—is discussed in Chapters V. and VI., and indirectly in other parts of the book. It has been my aim to show how meagre a conception of it is provided in the traditional logic, which assumes that the words used are free from ambiguity, and that every conclusion rests on a single generalisation. In actual argument ambiguity is seldom or never wholly absent, except just where no dispute or doubt arises. As we never infer one fact from another except by virtue of generalisation (tacit or express), so we always

(dimly or clearly) see the fact from which the inference starts as such and such a *kind* of fact ; and in seeing it so we put interpretations upon it. It is only so far as we have a perception (true or false) of the nature of the fact observed, that it leads our imagination backward to past times or forward to the future. Therefore, to find what theories underlie a given inference is to find at the same time how the observed fact is conceived, —what sort of description of its nature is regarded as satisfactory. False conception of the facts of a case is perhaps the subtlest of all sources of error.

Besides, it is very far from true that every judgment rests on a single generalisation. It is only true of the least disputable judgments. As a rule, a judgment rests on a large number of pieces of generalisation, better and worse in quality. Thus the notion chiefly to be guarded against, in the search for underlying theories, is that we can find them at once by mere inspection of the words

used,—just as, in the traditional logic, we can find a missing “premiss” with certainty. The reasons we can expressly put forward for our beliefs give always a somewhat incomplete account of the sources of our conviction; in order to find them fairly we must treat the record leniently, and give some play to our imagination. The search for underlying theories is thus always tentative and open to some correction as we learn more and more to read between the lines of verbal argument.

From Chapter VII. onward we are free to turn undivided attention to the nature of the process by which we generalise from facts observed and criticise our generalisations as they are formed. The results are summarised in Chapter XIII., and a few notes are afterwards appended for the use of students of Logic.

## CHAPTER II

### FALSE "FACTS" AND FALSE INFERENCE

IF we take any plain example of an error of "fact," it is easy to see what was meant, a few pages back, by saying that faulty *inference* from fact is all we need here discuss.

The clearest and simplest of all such examples may be found in conjuring tricks. We see, let us say, a watch destroyed by the conjuror, and the same watch is afterwards restored uninjured. Those who are acquainted with the ways of conjurors would of course not take the destruction seriously; they would know there was some deception. But it requires no great imaginative effort to put ourselves into the position of a less sceptical person—say a child—and to perceive how



strong might be that child's false belief that the watch was irretrievably damaged. The false belief would rest upon a "fact" that occurred before the very eyes of the child. From the fact as seen, the inference is correctly drawn. But the "fact" was as false as a fact can be—that is to say, it was misconceived. There is no other possible kind of falsity from which a fact can suffer; in every false fact there is a basis of real fact and a superstructure of faulty theory,—faulty *inference*. The watch was really handed to the conjuror; some object like it was perhaps really destroyed; but the falsity of the fact consists in our false inference (or theory) that these two objects were the same.

It is because of the intricate way in which fact and theory are mingled in all our so-called facts that we find it so difficult sometimes to make a satisfactory use of the common distinction between assertions of fact and assertions of theory. Whatever

value this division has, it does not enable us to set aside a *distinct* class of "assertions of fact" as above dispute. A vaguely-distinguished class of this sort we are all accustomed to recognise. There are plenty of cases where we all in practice agree that a settlement of a question may be reached by some short and easy method such as using our eyes, or referring to an authority. But to admit the existence of such cases, or even their frequent occurrence, is a very different thing from specifying exactly which they are. That no man can do, even to his own satisfaction. No clear line can be drawn between questions that are easy to settle and those where a settlement is not so easily reached. Whatever is seen or heard, is seen or heard in spite of obstacles either in the observer himself or in his surroundings; increase these obstacles—distance, for example—and the facts as seen or heard need more and more interpretation. Our state of mind, or of body, or the amount

and kind of our previous information, will often largely influence our sight and hearing. What one person will call the sound of clanking chains may be described by another, better informed beforehand, as due to a rusty weathercock ; what one person will call the sound of a muffled footfall on the stairs behind him, may be described by another, less alarmed, as due to a trailing tassel. And, from these weakest "facts" upwards the same possibility must be faced, so far as we care to criticise the judgment carefully. As soon as a fact is named, or described, or conceived in this or that way, it is seen in the light of theory, and the theory may be mistaken.

There is no need for us here to press this doctrine to its utmost possible extent, if that should seem unpractical. We may freely admit, for instance, that a normal person's eyesight and hearing are for the most part trustworthy. We are only concerned to notice the impossibility of drawing

any *clear* line of division between the class of questions which are easy to settle by appeal to the senses, and those which are less easy. This being granted, we may go on to observe that, so far as we aim at discussing the objections that may be raised against assertions generally, our interest lies rather with those "assertions of fact" which are nearest the doubtful line than with those that are farthest away; for it is precisely where truth and doubtful matter—fact and theory—are most intimately mingled that different opinions come into closest collision.

Almost any case of divided opinion, on a matter of history, would serve as a fairly typical example; for instance, the question whether Edison invented the microphone, or whether Byron ill-treated his wife. Such assertions evidently contain a mixture of admitted truth and doubtful matter. They have always a more and a less disputable part. There is no dispute, for instance, that Edison did invent something very like a microphone, and that

Byron acted in such and such a way. The doubt in the one case is whether Hughes' microphone was a "real advance" upon Edison's instrument, and in the other case how far Byron was to blame. The fair apportionment of blame in a quarrel is notoriously difficult; and since one invention always grows out of another, and contains the germ of many future developments, it is often hard to apportion the honour fairly. If we wish to discuss such questions, we must first separate their disputable from their indisputable part. We must admit something within them as "fact" while we class the rest as mere opinion or theory.

Precisely that is true of any assertion, whether its maker happens to call it a "fact" or not. In order to dispute it, we must regard it as theory based on fact; we must separate within it a more and a less disputable part,—a part which we are at any rate willing to admit to be fact *for the purpose of argument*, and another part which we declare to be mere faulty opinion. This does not

mean, of course, that we can never reject an assertion as "wholly untrue," but that when we do so we are no longer arguing. For the sake of argument a basis of fact must always be admitted. Before this is done, our objection is like the celebrated objection to Dr. Fell. It may be just or not, but our opponent has no opportunity of learning its justice—no opportunity of knowing even *what* the objection is.

In practice, assertions are seldom or never wholly untrue, especially when they correspond to a genuine belief. It is sometimes difficult, but seldom impossible (if it be thought worth while) to find a basis of agreement even with those whose view seems most opposed to our own. Some part of the way they go with us, and then the roads branch off. Why did they leave our road, or why did we leave theirs? That question and its answer is the beginning of argument. The matter of argument is always matter of opinion; not fact but theory; not fact but *inference* from fact.

That, then, is why a discussion of inference, its nature and its dangers, includes a discussion of all the definite objections that can be brought against a judgment. The *process* of objection is the same in every case. So far as we find any definite fault with a judgment, we regard it as a case where admitted facts are mistakenly seen or interpreted. Sometimes the assertion comes before us openly as a theory, but the process of criticism is the same where the assertion professes to tell us simple facts. It often happens that a so-called fact contains some highly disputable matter, and the whole examination may be stultified if we are content to pass this disputable matter as simple fact. The believer in ghosts, for instance, will sometimes in telling a marvelous story profess to keep entirely to facts, and "leave you to find an explanation of them for yourself," and yet his facts may be full of hidden theory. So far as we criticise them we criticise not the facts themselves but the

theories involved in them,—involved, that is, in the manner in which they are described, or conceived, or seen.

Whatever other subdivisions we may choose to make, for one purpose or another, within the class of assertions generally, the division into assertions of fact and assertions of theory covers all the ground. No assertion can be imagined which is not one (or both) of these. And, as we have seen, the so-called “assertions of fact” are a class that exist only on sufferance. Many assertions, disputable and disputed, are indeed called by their makers “assertions of fact,” because mankind are prone to mistake their own theorised facts for simple facts. And then what we find is that within the assertion itself a more and a less disputable part are always to be distinguished. This disputable part is the pith and centre of interest of any question. The first step in argument is to separate a kernel of admitted fact from a husk of unadmitted theory.



## CHAPTER III

### FOUNDATIONS OF INFERENCE

ALL our disputable judgments thus contain a mixture of fact and inference. They are only partly disputable. The disputable part we regard as based upon the indisputable part, as an "inference" either warranted or not warranted by the fact on which it rests.

The use of facts to-warrant inferences is very familiar—our habit of taking certain facts as signs of certain other facts; smoke as a sign of fire, for instance.<sup>1</sup> Our minds are full of such "generalisations," or grounds of inference, ranging from those of the most vague and tentative character upwards. And

<sup>1</sup> This example has the merit of antiquity. It was used in the typical syllogism of the Nyaya (Hindoo) philosophy.

always, in regard to any generalisation that we use, the critical question is, How far may we safely trust it, or under what conditions and limitations may it be taken as true?

Generalisations vary very much in the use that may safely be made of them. With some—like “where there is smoke there is fire”—hardly a fault can be found, for ordinary purposes; but with plenty of others it becomes a really difficult matter to decide how far we may trust them. Take, for example, the knowledge we have about classes of people,—say dissenters, or Jesuits, or peers, or working men. There is always some truth in the generalisations that are made about such classes, but their value as grounds of inference is often exaggerated; merely because a man belongs to a certain class we cannot with any safety infer that he possesses all the usual class-characteristics. Our ordinary loose generalisations have many exceptions, and in the absence of reasons for knowing the contrary any given

case may be an exception and not come under the rule.

Logic sometimes sets out from the fact that only a perfectly "universal" generalisation can properly serve as ground of inference; that if a rule be admitted to have even a single (unspecified) exception, its value for inference is lost, since any given case *may* be that one exception in the absence of knowledge to the contrary. And for some purposes<sup>1</sup> this view is useful. But we are now to look at another side of the truth, and one that has a closer connection with the actual process of argument.

It is comparatively seldom in actual argument—never, perhaps, where a really disputed or difficult question is raised—that we are able to rest our case on a single faultless generalisation, like "all men are mortal" or "where there is smoke there is fire." Inferences so supported are not in practice the kind that encounter opposition. Where any

<sup>1</sup> See Appendix, C.

doubt exists, our express or apparent ground of inference is, nearly always, a looser kind of generalisation ; we are obliged to make what use we can of broad truths which we know to be incompletely universal. Yet we are not, as a matter of fact, compelled to stop at the fact of their incompleteness. There is a roundabout way by which we can and do to a great extent remedy their defects.

This consists in the claim that for some reason the exceptions to the rule do not include the particular case in question. Although there are plenty of exceptions to the rule, for instance, that extravagance leads to ruin, or familiarity to contempt, yet now and then in a special case such faulty generalisations are used with confidence and safety, because we see both the rule and the special case in the light of other knowledge. The case in question is not merely "extravagance," but a kind of extravagance which specially deserves the name ; the money spent is not, we believe, in this particular instance

a means (say) of supporting credit and so of producing more income ; it is not a form of insurance against ill-health ; and so on. And in a similar way it is only under certain conditions that familiarity breeds contempt, and yet these conditions are capable of being approximately known.

Whenever we appear to use something less than a perfectly universal rule as ground of inference, that is, in effect, the way in which we justify our procedure. The generalisation as stated, or the generalisation that seems on the surface to be implied, is not by itself relied upon as sufficient, but we use it merely to suggest the true ground of inference, which is perhaps too complex to state conveniently in any short and telling form. The great majority of our commonest actual judgments, true and false alike, are based on evidence that cannot well be stated fully and yet concisely, if at all. What satisfactory account can be given, for instance, of the marks by which we recognise a particular

key on a bunch, or a friend in the distance, or somebody's footstep on the stairs? Such judgments as these are not, indeed, commonly matter of argument between two persons, but precisely the same sort of difficulty occurs even in the most openly argumentative matter. It is hard, for instance, to give a complete account of the reasons why we distrust our political opponents. The reasons we give, on a question so tangled, can only be a fraction of the real grounds of our judgment. When pressed to account for our faith, we often find it necessary to confess that even the best reasons we can put forward are sketchy, suggestive, and incomplete.

To this defence, however, the critic has an easy reply. If the ostensible ground of inference is not the real one, he says, he would be glad to know what the real one is. Till that is shown, the inference lacks necessity—may be right or may be wrong. It is unfortunate if the real ground cannot be

given, for then how can we possibly judge its value ?

Often enough a deadlock is reached in this way. Often no convincing reason for a belief can be stated, and yet the believer remains convinced, rightly or wrongly. But in the fortunate cases where some progress is made by means of criticism, such progress depends on the fact that more and more of the real ground of inference is stated, or made ostensible ; and this may take place in several different ways.

One way is *closer specification of the rule* ; that is to say, a more definite statement of the class to which the generalisation is meant to refer. Not every kind of X, we grant, permits the inference of Y, but only some kinds ; or X under certain conditions only. Our notion of the character of any class is nearly always too sweeping at first, and gradually becomes less so, as we learn to discriminate within the class. Sometimes, even after the process of discrimination has begun, we con-

tinue to use the wide class-name for brevity, as where we say *familiarity* breeds contempt, though we only mean some kinds of familiarity, or familiarity under certain conditions. But we may also use the wider name through ignorance or carelessness. For we often get an idea—it may be only from novels, or caricatures, or plays—of the typical form of a class; say, the typical worldly woman, or family lawyer, or curate, or *parvenu*; and then we are apt to use the wide class-name when we are thinking only of these selected and pungent specimens. And the criticism which forces us to recognise the number and kind of exceptions to our ostensible generalisation performs in this way a useful service to the cause of truth. The more careful statement of the rule helps us to see whether the case in question belongs to the rule or not.

But closer specification of the rule is the simplest, and perhaps the least usual, effect of criticism. Perfectly universal generalisa-



tions are so seldom to be reached, even by narrowing the class referred to, that very often the challenge to produce the real grounds of belief is met in one of two other ways,—either by reference to *analogy* or by adding fact after fact, each by itself perhaps insufficient for proof, but important when taken together. This latter is sometimes called *circumstantial evidence*.

Analogy and circumstantial evidence have much in common with generalisation, and with each other; and I freely admit the impossibility of drawing firm lines between them. Rather than pretend to do so, we may here adopt the opposite method, of seeking their points of likeness, and so of regarding each as a form to which any grounds of inference may on occasion be reduced. But first let us try to make sure that no unnecessary misunderstanding remains as to what has been said already.

The main purpose of this chapter has

been to notice the nature of "grounds of inference" as they are used and put forward in daily practice. Inferences always profess to rest upon facts, and the use of a given fact to warrant a given inference is a process we can all observe with sufficient ease. What we find is that our minds are stored with bits of generalisation,—more or less trustworthy as the case may be,—and that these, in some shape or other, lie at the root of all the inferences we draw.

I say "in some shape or other," because very slight acquaintance with actual inferences is needed to show the rarity (and the unimportance in regard to disputed questions) of the case where we rest a judgment upon any single generalisation, sharp and self-contained and true,—like "where there is smoke there is fire." In practice our stores of generalisation are mostly of a lower order of certainty than this,—often mere glimpses of the way things hang together, or mere outlines which, all our life, we shall be

engaged in filling in. Yet they serve us for inference somehow, and indeed if we sat down in despair and declined to use them because they fall short of perfection, few and flat would be the truths we should attain.

Rough general rules are what we mostly use for inference. But there is all the difference between the ways in which the dull and active mind will use them. The duller we are, the more blindly we shall accept them as trustworthy throughout ; the keener our intelligence, the more we shall learn to notice when they may and when they may not be trusted. And in practice the judgments of the average man are full of such discrimination ; full of regard to the "special circumstances of the case,"—the extra facts, beyond those expressly put forward, which help to justify the inference.

This means, in short, that our *actual* grounds of inference are usually composed of a number of pieces of generalisation, which may or may not be capable of being

fairly expressed in a single formula. As a rule our inferences are a little better grounded than they are apt to seem when we try to state their grounds expressly. The facts on which we base a judgment are always fuller of detail than our best description of them shows; and some of these extra details are seen as a rule by the man of common sense when he forms a judgment.

Just here lies the value of criticism. Its tendency is to force the extra details—the special circumstances of the case—into open view. As already noticed, there is always a chance of a deadlock arising between assertor and critic, if they fight for victory rather than for truth. When the real grounds of a belief are very complex, or require fine artistic or moral perceptions which are only partly conscious, what is the gifted seer to do when a hard and narrow critic demands to have the whole process concisely explained? Or, on the

other hand, what is the modest and inquiring critic to do when a vague and pretentious assertor takes refuge in the plea of artistic insight too lofty to be imparted to the vulgar? Each can, if he chooses, in some such way make a plausible claim for his own point of view, and remain there undisturbed. But fortunately there are in practice—through the general modesty of mankind—very many cases where the deadlock does not arise, but where the demand for grounds of belief leads us gradually into clearer light. It is the process which takes place on these occasions that we are now attempting to explore.

## CHAPTER IV

### GENERALISATION, ANALOGY, AND CIRCUM- STANTIAL EVIDENCE

IN the preceding chapter I have used the name "generalisation" as if there could be no difficulty about its meaning. In many cases there is no such difficulty. To a great extent the glimpses of natural law which serve us in drawing inferences are just what every one commonly means by "generalisations," or bits of generalisation; and perhaps if we were forced to find any single descriptive name by which to speak of grounds of inference generally, no more convenient name could be suggested. But ordinary language, as we find it used, is not so uniform, or so limited in resource, as to allow a single name,

for so wide and varied a class of cases, to drive all other names out of the field. It is rather the maker of technical terms, than he who adopts the language his neighbours commonly speak, who would try to extend the meaning of a word like "generalisation" to cover cases that require some effort to bring under it. And the reader is not at present asked to make any troublesome effort to extend the term generalisation except so far as he himself may find a convenience in doing so.<sup>1</sup>

As remarked already,<sup>2</sup> the grounds of an inference would often naturally be described in some other way than as generalisation. We should commonly say of certain inferences, for example, that they are based on *analogy*; of others that they depend on *circumstantial evidence*. If any other kinds or modes of inference may be distinguished, they can easily be made to come under one

<sup>1</sup> In Chapter VII. we shall find an occasion where this convenience may arise.

<sup>2</sup> Page 29.

or more of these three heads. But it would be a mistake to draw any firm dividing-line between these kinds themselves. The division is a convenient one and no more. At bottom all three modes of inference are of the same nature; the differences between them are differences of degree; and though in some respects they are important differences, in other respects they have no importance at all.

For just as the word "generalisation" may with a little straining be applied to the grounds of inference in every case, so we might (with perhaps a little more straining) use the term "analogy" or even "circumstantial evidence" for the same purpose.

In the end, all proof depends upon our seeing analogies, and to rest upon fact at all is to rest upon circumstances. If we are able to show, or to perceive, the reasonableness of drawing this or that inference from a given fact, we must be able to refer somehow to



analogous cases,—cases where a similar<sup>1</sup> result has followed or accompanied similar facts,—and where accordingly the inference would have been justified by the event. If I judge, for instance, that a given attack of illness is likely to end fatally, I can only ground the judgment by referring somehow to similar cases which have had a fatal ending.

But in that “somehow” lies the difference. When we can refer to the similar cases under a (more or less) definite class-name,—say *cancer* or *consumption*,—then we naturally regard the judgment as based on a generalisation; at present it is thought to be a trustworthy general rule that cancer and consumption are incurable. But when, owing to the great complexity of the circumstances, or the rareness or unfamiliarity of the case, there happens to be no single class-name under which we can satisfactorily bring it, then we

<sup>1</sup> “*Essentially* similar” would be more strictly correct, but the discussion of the meaning of this phrase is left over for the present. See p. 56; Chapter XII.; and Appendix, E.

incline to describe the judgment as based upon analogy. Sometimes, for example, the doctor cannot see his way to give the disease a definite name ; yet his wide experience of other cases helps him to form as strong and correct a judgment, though in a less consciously reasoned manner. Or we want to judge, say, whether Panama Canal shares are likely to be a good investment, but since no single "generalisation" seems exactly available,—the making of large ship-canal being a rare event,—we are thrown back on the few analogous instances that occur to us ; and in fact the success of the Suez Canal did, in this way, mislead a good many people.

We need not here raise the question as to the advantage or disadvantage that inferences of the analogical type have as compared with those that rest on an express generalisation. It is a very complicated question,—like asking whether poetry or science on the whole comes nearest to the truth. At

present we are trying to see what is meant by calling the distinction a merely convenient one, not completely hard and fast. In both forms of inference alike, then, our source of confidence is the resemblance (or "analogy") we discover between the case in question and other cases where the event has already occurred. But sometimes we can, more or less easily, use a general name to cover the class of similar cases, while at other times we are more or less hindered in finding a name that will serve. Accordingly, to see an analogy is to generalise, in a less express and definite form; and we never generalise except where we see an analogy. It requires, however, some troublesome straining of the ordinary sense of the word analogy, thus to identify it with class-resemblance; and that is why in the former chapter I preferred the word generalisation, as being perhaps in common usage a little wider. *Cancer, consumption, animal, man*,—examples of any one of these we should not commonly call

“analogous” to each other, but rather members of their respective sorts or classes. Where analogy is very close, and well tested, and familiar, as between cancer and cancer, or man and man, class-names have generally been invented. It is newly-seen likeness, doubtful likeness, or likeness where the examples are rare, that we have to recognise as well as we can without the aid of class-names. And it is to these kinds of likeness especially that, as a rule, we give the name “analogy.”

It is a little less easy, at first, to see the close connection that exists between the inference from analogy (or generalisation) and that which rests on circumstantial evidence. The likeness between them is apt to be hidden from us by our habit of using the name circumstantial evidence specially in inferences where both the analogous facts and the generalisation are apparently absent,—where it seems that, just because we have no general rule which exactly covers the case, nor any clear precedent to go by, we are thrown back

upon a different and perhaps more risky method of proof. But though difficult to see, the connection may nevertheless be detected. For the details or circumstances of any occurrence are exactly what the essential nature of that occurrence depends on, and are therefore what determine the extent of its similarity with other occurrences,—its right to be classed along with them; and the more of such details we can find, the better shall we see both the peculiarity of the case in question and its “general” aspects. The more we know of the peculiarities of a case, so much the more we know in what respects it is *not* peculiar; since each of the peculiarities, or special features, if it has any meaning for us at all, obtains such meaning by virtue of analogy or generalisation; each detail, so far as it has any value, refers to a set of precedents.

This truth will become clearer if we keep in view any actual instance of what every one would call circumstantial evidence. I suppose

the name is nowhere more commonly used than where a crime is traced to its author by means of the marks he has left behind him,—for example, his footprints in the clay or snow. But what is it that gives importance to the fact that the boots of the prisoner fit the marks? Simply our knowledge of the way things happen in Nature, which may enable us to make a sufficient number of bits of-generalisation bearing upon the case. We assume that the clay, and the other things, have behaved on this occasion exactly as we have known them behave before. Clay, of a certain consistency, *always* takes an impression, and the impression will *always* correspond in certain ways with the boot that made it, while in other ways the correspondence may be less exact without condemning the inference. If rain falls afterwards, some of the details will perhaps be blotted out ; and so on. Thus every smallest circumstance that we regard as relevant gets its relevance through our knowledge of the

regular ways of Nature. And so, when we judge of a case directly by means of its details, it is only an accident that we can neither expressly classify it nor consciously compare it with certain other remembered cases. Tacitly and unconsciously, at least, we generalise and use analogy whenever we interpret any facts as having a meaning. In using circumstantial evidence, as in using analogy, we combine as best we can a number of pieces of rapid and rather unconscious generalisation.

No doubt, in the argument from circumstantial evidence we appear to make direct use of our knowledge of causes and effects, without stopping either to classify the special occurrence under a general name or to bring definitely to mind other occasions on which the same fact has had the same meaning. That is, no doubt, one reason why we make a distinction, such as it is, between the argument from circumstantial evidence and the other kinds already noticed. But without

in the least disputing the value of the distinction for certain purposes, our object at present is to over-ride it, in the search for a broad and simple notion of the nature of inference,—a notion under which *all* grounds of inference will show their essential likeness to each other.

The general view we get, then, of the process of inference in its three chief forms, is that it never takes place without some knowledge (or supposed knowledge) of the way things and qualities and events are connected, in pairs or groups or chains, by natural law. Sometimes the case appears so simple and straightforward that we are able to give it a class-name and so bring it under a “generalisation.” Diseases and death, human nature and fallibility, and many other pairs of things, hang together so closely that we can formulate our knowledge of their connection concisely in a sentence. But in proportion as the judgment appears compli-



cated—in proportion as the case appears to be *sui generis*—we are compelled either to mingle with our main generalisation (say “Familiarity breeds contempt”) other subordinate ones which bear upon the question, or else to do without any single main generalisation and trust to a multitude of smaller ones, each by itself perhaps exceedingly insecure. In the argument from analogy we put together a number of little points of likeness between case A and case B, and (rightly or wrongly) judge the total likeness sufficient. The Panama Canal resembled the Suez Canal in various evident ways—amongst others, in being schemed by M. de Lesseps; and, no doubt, among other bits of loose generalisation that the unlucky investors put together, was one to the effect that “whatever M. de Lesseps undertakes is likely to succeed.” Similarly in the argument from circumstantial evidence. The special circumstances of every occurrence are so many marks, or signs, or indications,

and get their significance from our knowledge of the laws of Nature generally. Whether the given inference be right or wrong, whether it be express and deliberate or rapid and free, whether it take the form of a cut-and-dried Syllogism,<sup>1</sup> an argument from analogy, or from circumstantial evidence, in all cases equally it is our beliefs about the way things hang together in Nature that provide alike the sole motive power of inference and the sole foundation on which we rest our proof.

<sup>1</sup> See p. 62.

## CHAPTER V

### THE DISCOVERY OF UNDERLYING THEORIES

WE have seen, in Chapter II., that every objection against 'the truth of a reasoned judgment resolves itself into an objection against an inference from admitted facts. In Chapters III. and IV. we saw that this again resolves itself into an objection against the underlying *grounds* of inference, and that they are wholly composed of pieces of theory about the way such and such things (or qualities, or events) are connected by natural law. If, therefore, we have any fault to find with a given judgment, it must be capable of being reduced to a fault found with some one or more such pieces of underlying theory.

The art of criticising reasoned judgments,

then, depends partly on our power of discovering the theories which underlie them, partly on passing these through the fire of criticism so that they may be consumed or purified. Both processes admit of great variations in the skill and refinement with which they are performed ; and if our aim be to find always the fairest and most useful objections, it will be worth our while to distinguish as well as we can between the better and worse performance of these two fundamental operations. The two departments of study cover nearly the ground occupied by what are usually called respectively "deductive" and "inductive" Logic.

As already suggested, it would as a rule be unfair to assume that when a fact X is appealed to as evidence for a conclusion Y, this points to a "ground of inference" connecting X and Y *universally*. It is only the simpler and less disputable inferences that can fairly be so explained—such as the inference that Socrates, being a man, is mortal.

The more complicated and difficult the question disputed, the more probable it becomes that behind the reason expressly put forward lie a number of "extra details," seen by the assertor and adding force to his belief.

Take, for example, that very common form of argument where a person (or thing) is judged to have the quality Y on the ground that he has the quality X, or belongs to the class of X's—say where Smith is judged to be meritorious because successful; to be respectable, or stupid, because a conservative; to be guilty of some crime because a gainer by it. On their face, such arguments seem to point to grounds of inference like "success is a sign of merit," or "all conservatives are so-and-so," or "those who gain by a crime will commit it." But in such cases it is a very common experience to find, on further inquiry, that the arguer does not as a matter of fact believe that these generalisations are strictly true. Merit, he will freely admit, is only one among several

alternative causes of success ; or the conservatives may be, broadly speaking, the stupid party, but there are many exceptions ; and though an act must have a motive, the motive does not always lead to action. When we judge that Smith's success is due to his merit, we commonly have in view plenty of other knowledge about Smith than the mere fact of his success ; at the very least we see him in the light of several other epithets (*e.g.* industrious, sensible, etc.), and so we judge that although success in general cannot be taken as a quite satisfactory sign of merit, yet Smith's success is precisely the *kind* that can be so taken.

Beginners in Logic are even more apt than those who have never begun the study to commit the unfairness, in actual argument, of overlooking these troublesome considerations. The first view we get of Logic is generally much too abstract, too hard and fast in its conception of the reasoning process. The forms of argument that we learn

from our text-books are far too simple for direct application to actual pieces of reasoning,—except of the indisputable kind ; but this fact is rather hidden from us by the traditional doctrine, and has to be rediscovered when we get beyond that study. There seems no reason, however, why we should not at once begin with the recognition of it, especially since it is already seen with some clearness by many whose only Logic is provided by common sense.

The practical difference this recognition makes in the search for underlying theory consists chiefly in leading us to regard that search as tentative and hard to finish, rather than to end it as quickly as we can by catching at our opponent's faulty expressions. Instead of triumphantly finding that, since X is given as a reason for Y, the assertor is thereby committed either to the untrue generalisation that X in all cases indicates Y, or else to a faulty syllogism,<sup>1</sup> we

<sup>1</sup> See pp. 63-65.

take the fairer line of supposing his statement of the reasons incomplete, and requesting him to add whatever details he can for the sake of convincing us. That is the form the inquiry takes where the judgment criticised is our own, and there is no reason—except the desire for an easy controversial victory, right or wrong—why we should not in this matter treat our neighbour as ourselves.

As noticed already, the result of the inquiry, where a deadlock is avoided, is to bring to light the special circumstances of the case, and so to build up a completer knowledge of its essential nature. We shall presently see that the notion of “essence” plays a great part in the formation and criticism of all opinion or theory as to the way things hang together in Nature, and meanwhile there will be some use in observing how each of the three main types of inference—by generalisation, by analogy, and by circumstantial evidence—depends to



some extent upon a ready-made view of the "essence" of the case considered.

Where the ground of inference is a generalisation,—that is to say, an assertion about a *class* of cases,—any case where that generalisation is applied is necessarily viewed as a member of that class. We cannot apply, to a given man, the general knowledge we may possess, *e.g.* about Eurasians or atheists or madmen, unless we believe him really to belong to the class in question. The individual case is labelled with a *general name*, or class-name, and because the label is supposed to be correct the generalisation is supposed to be applicable. A great many general names—*man*, for example—can be applied to special cases, nearly always, without any doubt arising as to their correctness; but wherever such doubt arises,—say in the diagnosis of a perplexing illness,—we become conscious that naming a case involves some theory as to its nature. An immense amount of argument, or doubt, is for ever playing

around our application of names to things or people or cases. The things we regard as members of certain classes have a way of possessing points of likeness to several classes at once, or of changing their nature according to changes in their surroundings. The apparent atheist may (like Spinoza) belong also to the class of religious men, and so come under another and a conflicting generalisation. The hard man of business may be easily ruled at home by a favourite daughter. A movement in public opinion is not always easy to provide with a fitting name. Where this difficulty occurs, it is plain that to single out some one name as especially or "properly" applicable involves an opinion as to the deeper nature—the *essence*—of the thing so named; and where the difficulty does not occur, it is only because we are all agreed in our theory, and because accordingly the contrast between the superficial and the deeper nature happens not to come into view. Such contrast can

seldom be troublesome in the case, for instance, of a name like *man*, but it does produce trouble just wherever any difficulty in defining is practically felt. The "essence" of vice or virtue, of pleasure or pain, of truth or untruth, of beauty or ugliness, provides almost endless matter for difference of opinion, and there are few descriptive names whose application is secure against ever becoming a source of difficulty. But whether difficult or not, in a given case, the argument by means of a generalisation always, as such, involves a theory that the case is *rightly* described by the general name in question, or belongs *essentially* to the class.

In the argument from analogy, theory as to the essence of the cases compared is even more openly referred to. The perception of analogy is, as explained in Chapter IV., an earlier stage of the process which ends in the adoption of a class-name, or is a substitute for that process where a new class-name is not called for,—as where the analogy is

extremely complex, or the cases observed are rare. As a rule, in referring to analogous cases, we are a little more conscious, than in using a ready-made class-name, of the theoretical (or disputable) element in what we are doing ; to dispute an analogy is often an easier task for the critic than to find fault with a name as applied. At any rate the notion of "essence" is in this form of argument very plainly important ; the claim that is always made by the supporter of an analogy is that the cases are (for the purpose in hand) "essentially" similar, while the disputer's claim is that they are (for that purpose) "essentially" different. We cannot therefore either use an analogy, or decline to use it, except by means of theory as to the essence—the deeper nature—of the cases compared.

In the argument from circumstantial evidence we are at our farthest from the danger of begging the question by means of a name. Ready-made theory as to the nature

of the case is here less dominant. In this form of argument we know, at least, that the problem is to put together our notion of the essential nature of the case carefully and piecemeal, and to that end the whole investigation is directed of set design. Yet even here we are not wholly unbiassed. It is by means of pre-existing theory that we distinguish between important and unimportant circumstances, or between the circumstances which really form part of the case,—are essential to it,—and those which are outside it and “accidental.” And here, just as in the other modes of argument, the influence of theory is only felt so far as doubts and difficulties arise in drawing the line between important and unimportant circumstances. Here also, therefore, the effect of criticism is to make us aware of underlying theory, as a first step towards its improvement or subversal.

The purpose of referring thus to the

underhand action of ready-made theory, in the three chief modes of inference, is to throw some further light on the process of discovering the special assumptions that underlie any reasoned belief. We may at least see in this manner how insufficient a conception of its difficulty is given by any logic which is content to show the ways in which terms are put together to form arguments when the terms are regarded as free from ambiguity. In actual argument—the simplest and least interesting specimens excepted—ambiguity of terms plays a very important part. The question how far a so-called case of X really deserves the name, or how far the resemblance between it and other cases is “essential,” or what circumstances really form part of its essence, and so “belong to it,” is an effective source of error wherever opinion is much divided, and is naturally the more effective the less we suspect its presence.

Completely to discover the theories that underlie a given belief may be an endless

quest; yet it need not on that account be neglected as hopeless, or simplified by leaving its difficulties entirely out of sight. It is not the *improvement* of our judgments that is hopeless, but at most their attaining to absolute perfection. We may not be able to find all the underlying theories in a given case, and yet so far as we can find them we are helping forward the process of criticism. What is important is that as much as possible of the underlying theory should be brought to light, and so the real ground of the inference stated as fairly as possible. There is a double risk of unfairness: on the one hand the easy victorious plan of catching the assertor in a net formed by his own incomplete expressions; on the other hand the still easier but less victorious plan of allowing him to beg his question by means of a piece of hidden theory. The fair course lies between these easy methods.

## CHAPTER VI

### SYLLOGISM, OR INFERENCE IN THE ABSTRACT

THOSE who have been accustomed to think of Logic as a wordy business may perhaps dispute our right to use the name to describe the subject of this book. They may consider that we are here at most concerned with "inductive" Logic, and that we have left "formal" Logic almost entirely out of account. As was mentioned at the beginning, I am not concerned to argue for or against any particular view of the proper province of Logic, but would rather yield the point to any one who cares about it, and keep to our own concerns. Yet, for some readers at least, it may now be worth while to notice briefly the relation which "Formal Logic" bears to the general theory of Inference.



The actual process of Inference from facts, so far as we have succeeded in getting a general view of it, appears for the most part one of great complexity. Most of our actual inferences, that is to say, are plainly a tissue of smaller inferences which are somewhat vaguely conceived, rapidly drawn, and loosely put together. X is thus spoken of as a sign of Y in a particular case, not because X in general (*i.e. in all cases*) can be relied upon as indicating Y, but because the various parts or circumstances into which we analyse this particular case in question—which is X and something more—are supposed, when taken all together, to make up the required indication. It was in order to give due weight to this truth relating to actual inferences that we have departed from the more usual method of considering inference in the abstract ; but at the point now reached there will be no harm in taking for a moment this simple view of it.

Inference in the abstract—or the unit of inference, as it may be called—is precisely

what the traditional Logic tries to describe under the name of *Syllogism*. It is also the process that occurs in those rare and (controversially) unimportant cases when we can use a single trustworthy generalisation as ground of inference, instead of relying to some extent on the "extra details" of the case. In itself the process is familiar and easy to understand; but, since mistakes may occur in the interpretation of the *sentences* used in expressing the inference, the traditional Logic has spent much trouble in reducing to rule the means of avoiding such error. We have already decided, for reasons given above, not to trespass upon that province of the subject, but shall be content here to notice the parts of which any syllogism is composed.

Syllogism may be described as the process of combining a fact and a ground of inference when no doubt is raised as to the truth of the one or the trustworthiness of the other. And the formula :

Ground of Inference or <i>Major Premiss</i>	} M indicates P	{ (e.g. Where there is smoke there is fire).
Fact relied on or <i>Minor Premiss</i>		
Conclusion		
	S is M	{ (e.g. Here, there is smoke).
	S is P	{ (e.g. Therefore, here there is fire).

is typical of the Syllogism in its most general form. Under all variations<sup>1</sup> these three parts are distinguishable. In a valid syllogism the conclusion is always implied in the premisses—that is to say, contained in the *meaning* of the two premisses when taken together. And, therefore, the conclusion and either of the premisses together determine the other premiss required.

If our general view of the actual process of inference is correct, it follows that, even where we have good reason to regard a given inference as faulty, it is seldom easy to say whether the fallacy is a “syllogistic” one or otherwise. It is rare, in practice—as we have said so often already—for an inference to rest on any single syllogism ; and

<sup>1</sup> See Appendix, D.

even where, on the face of it, this seems to be the case, it is exceedingly rare to find both premisses of any syllogism expressed. As noticed in Chapter V,<sup>1</sup> what usually happens is that, in defence of a conclusion, some reason is given which looks as if it might be a premiss; and that if we assume it to be a premiss, we do the arguer some injustice. Even if we are right in assuming, in a given case, that a single syllogism is relied on, we cannot tell, before some further inquiry takes place, whether the arguer wrongly believes in the truth of the sufficient ground of inference, or rightly believes in the truth of the insufficient one,—in the truth of some other generalisation, nearly resembling the required ground of inference, and yet not equivalent to it, and so insufficient to establish the conclusion. Only in

<sup>1</sup> P. 49. Of arguments which seem like simple syllogisms with one premiss expressed and one left tacit, perhaps the commonest form is "S is P, since S is M." That is to say, we argue that S (the subject) has some one quality, P, since he (or it) has another quality, M.

the latter case can the fallacy be located in the Syllogism itself.

Nevertheless it will perhaps be useful to ask how, supposing purely syllogistic fallacy does sometimes occur, it is most probably occasioned. The broad answer to this question has been already suggested. Syllogistic fallacy, so far as it occurs at all, occurs through misinterpretation of sentences. Whenever an invalid syllogism is accepted as valid, some two sentences have been wrongly interpreted as equivalent to each other. In no other way, surely, can we be deceived as to whether a given conclusion is implied in given premisses, since implication is entirely a question of meaning. But if we try to make clear to ourselves the exact manner in which the mistake is caused in all possible cases the problem becomes more difficult.

Among the various sources of confusion as to the meaning of a sentence, there can be little doubt that *ambiguity of terms* holds

a foremost place. And when we have done our best to guard against this,—a matter in which the “Rules of the Syllogism” give us no help,—the mistake that is probably of next importance is that of faulty “conversion.”<sup>1</sup> The supposed difficulty of converting sentences properly is the chief reason for the existence of the old Rules of the Syllogism, and it is therefore worth while here to ask how far the difficulty may be considered in practice important.

The traditional method of dealing with this source of error is to lay down certain rules for conversion, and to declare that any case of conversion that does not conform to those rules is faulty. To such a plan there are two chief drawbacks—(1) that it involves

<sup>1</sup> To “convert” a sentence is to change the order of its terms, so that the “subject” becomes the “predicate” and *vice versa*. A sentence (or “proposition”) is said to have undergone “simple” conversion when no other change of form is made in it simultaneously. Thus the change from “All X is Y” to “All Y is X,” or from “Some X is not Y” to “Some Y is not X,” would be called simple (and faulty) conversion. The change from “No X is Y” to “No Y is X,” or from “Some X is Y” to “Some Y is X,” would also be called simple conversion, but is not faulty.

the previous reduction of all assertion to a few forms of sentence,—much fewer than ordinary language in practice uses,—and it is hard to believe that human beings are any less fallible in performing this reduction than in taking the further step of conversion itself; and (2) that it does hardly anything towards pointing out the occasions on which a mistake is most likely to occur. In the eye of the traditional Logic all cases of faulty conversion are equally faulty conversion, and there is an end of them; at least no express distinction is attempted between those which are plausible and therefore dangerous, and those where the danger is practically non-existent or trivial.

A better way seems to be to raise the question, What leads us to convert at all? We may, of course, play with sentences as a child with bricks, merely trying how far we can re-arrange them.<sup>1</sup> But the process

<sup>1</sup> Thus Jevons (*Studies in Deductive Logic*) sets the question, How should the proposition "It rains" be converted? and

has also a practical justification, which can best be understood in the light of the notion of "grounds of inference" and their use.

And first, though a ground of inference is always *the right of inferring from some fact*, the fact so inferred from may be either the presence or the absence of a quality, the truth or the untruth of an assertion. Thus we may infer the presence of returning health from the absence of such and such symptoms, just as well as we may infer the presence of an illness from the presence of those symptoms; and we may draw inferences from the untruth of the assertion that the angle at A is equal to the angle at B, just as well as from its truth.

Secondly, it lies in the nature of grounds of inference that every one of them gives us *two* facts as warranting two different inferences. There can be no such thing as

gives as the answer, "Something which is letting rain fall, is the atmosphere." No wonder students of elementary logic (and still more the general public) are sometimes puzzled to discover the practical value of the study.



a ground of inference which is purely one-sided. If the presence of X indicates the presence of Y, then the absence of Y indicates the absence of X.<sup>1</sup> Hence, to "convert" a ground of inference is no more than to see both its aspects at once; and the use of the process is obvious, since the absence of a quality (or the untruth of an assertion) attracts our notice in practice quite as frequently as their presence or truth, and the sentence expressing the ground of inference may come before us in either of its two different shapes. We "convert" a sentence expressing a ground of inference, therefore, whenever we use it in spite of the fact that it happens to be given to us (or to occur to our mind) with its wrong end foremost. Thus if we argue :

<sup>1</sup> It is a little difficult to state this perfectly general truth in a sufficiently general manner. It applies to the *relation* between the terms, independently both of the question what the terms are (*i.e.* whether presence or absence, truth or untruth, of anything), and of the question whether the indication is asserted or denied. The one rule for converting an indication may be stated—*Reverse the order of the terms, and "contradict" both of them, leaving the assertion*

Ghosts do not speak,  
This apparition is speaking,  
Therefore this apparition is not a ghost,

we may be said to convert the ground of inference (or major premiss) as given, into "What *does* speak is *not* a ghost," however tacitly or unconsciously the process be performed.

There can be little doubt that the conversion of grounds of inference is in practice not only the chief use of conversion but the chief occasion on which conversion is wrongly performed. Certainly, the relations between classes (inclusion or exclusion) are also two-sided, and so far as our minor premisses<sup>1</sup>—or statements of fact from which to infer—are statements of the relation

*or denial as it was.* Thus if (*e.g.*) it be asserted that the absence of X indicates the presence of Y, it is also thereby asserted that the absence of Y indicates the presence of X; or if it be denied that the presence of X indicates the presence of Y, then it is thereby also denied that the absence of Y indicates the absence of X.

<sup>1</sup> When a *major* premiss is, on its surface, a statement of class-relationship, it is nevertheless as an inferential assertion that we use it.

between classes, they also may happen to come before us with their wrong end foremost, and so to require conversion. And no one can say that sentences expressing class-relationship are free from all danger of being wrongly converted. Still, it may fairly be argued that mistakes of this sort are far less plausible, and therefore less dangerous, so that for the purpose of our present discussion they deserve a very subordinate place.

What especially makes the conversion of "inferentials" a source of error is the fact that a certain proportion of them are reversible without contradicting the terms (or the terms may be contradicted without reversal of order). Thus equal-sidedness, in a triangle, is a sign of equi-angularity, inequality of sides a sign of inequality of angles, equality of angles a sign of equality of sides, and inequality of angles a sign of inequality of sides. Moreover, even where this reversibility is not complete, the inquiry how far (or on what occasions) it fails of completeness

is often interesting and useful ; so that the assumption that it is complete is often tempting and natural, and sometimes justified. This, in fact, is the chief reason why it is so difficult to decide in given cases that a "syllogistic" fallacy has been committed ; the fallacy is not a syllogistic one unless the two assertions have been wrongly *taken as equivalent*, whereas the actual mistake may lie in the false assumption, not that they are equivalent, but that both are true.

It may also be remarked that plausible examples of syllogistic fallacy are not very easy to find. One reason, doubtless, is the fact that we do not nowadays commonly set out our syllogisms in full, even where an argument really depends on a single syllogism ; so that any such example is liable to seem pedantic. Another reason is that so many assertions do not run naturally into any of the few forms of sentence recognised by the traditional Logic. But, apart from these difficulties, the text-books generally

introduce a needless amount of unlikelihood into these examples, by taking propositions which are uninteresting through being too obviously true (*e.g.* "No dogs are men," "Some coins are metallic," etc.), or which at best become too plainly absurd when wrongly converted. In practice, for instance, "All men are fallible" and "All fallible beings are men" are never in danger of being regarded as equivalent, since the latter is plainly untrue; and so a syllogism with "All men are fallible" as an insufficient premiss does not in practice occur. If we are to find cases that seem plausible, we must use only such assertions as, besides being in themselves likely, have some likelihood when simply converted; for example, that all conservatives are stupid, all radicals rash, all acts due to a motive, and so on.

On the whole, therefore, we may be content, when discussing the nature and the dangers of the process of inference, to give an extremely subordinate place to the

doctrine of Syllogism, and to Formal Logic generally. Whatever study best corresponds to the old name of Logic, or provides the best training in grammar, or simplifies most the art of setting questions which shall test a student's industry, the subject that is usually taught as Logic gives little or no help in regard to the actual difficulties of reasoning, or as to the sources of the more plausible kinds of error and verbal confusion. As a historical study it may have great value—for the few who have time to pursue it as a part of the general history of philosophy. As a mental exercise it may or may not be as good as a game of chess. But for the purpose here kept in view—of gaining some insight into the distinction between sound and unsound inference—it is an open question whether the good or the harm it does is greater. Taken as a whole, it is an accumulation of odds and ends that have survived from various outgrown philosophies ; and, so far as it does deal with the distinction

between sound and unsound inferences, it introduces an artificial simplification of the difficulties in too unyielding a form. For it tells us only what the soundness of inferences depends upon when we assume that the words in which they are expressed are free from ambiguity. In actual inferences this assumption is never strictly in accordance with the facts, and is least in accordance with them when the soundness of the inference is most debatable. That is the chief reason why an appeal to Syllogistic Logic is generally so unconvincing. Now that the direct inquiry into Nature is open to almost every one, almost every one has begun to learn that sharp-cut words are traps for the unwary. A Syllogism can always be blocked by refusing to admit the truth of a premiss, and in these times no special study of the forms of Syllogism is needed to show us (in practice at any rate) that the easiest and most effective way to do this is to criticise the words in which it is expressed. Where the

conclusion is disputable there is seldom any difficulty in finding some want of definiteness in the premisses, so that they can only combine to form a conclusion when one of them is interpreted in a sense which makes it untrue. To raise this objection—in however untechnical language—is to tell the syllogistic logician that his simple process is not yet available. The real difficulty of the question has first to be settled, and then those who care to do so may put the reasoning into “syllogistic form.”



## CHAPTER VII

### KINDS OF GENERALISATION

THE pieces of theory which are woven together to form the ground of an inference are, as we noticed in Chapter III., of the nature of generalisation. If, in keeping as near as we can to the ordinary use of language, we should hesitate to apply the name generalisation to some of them, as being too unformulated to deserve it, at any rate no clear line can be drawn in practice between those that deserve the name and those that do not. We cannot fix the point at which a mere glimpse of regularity in Nature—a suspected analogy, for instance—becomes definite and coherent enough to rank as a generalisation. For that reason, and for

another to be presently mentioned, we shall here find it convenient to extend the term generalisation a little, or rather not to insist on any sharp distinction between fully-formed generalisations and those which are only partly formed.

The special reason why this extended use of the term is here required is that our immediate aim is to understand as far as we can the way in which generalisations take shape and grow ; for the principle of growth that is in them is precisely the most interesting quality they have when our object is to study the sources of their strength and weakness. We are in no way tied to use the name generalisation if a better one can be suggested. All that we want is some one name which shall be taken widely enough to include any embryo forms of generalisation which it may be worth while to notice. For we have here to consider not only our *knowledge* of natural law and of the meaning of facts, but the guesses and glimpses that

prepare the way for such knowledge. That some of our "generalisations" are better developed than others, no one disputes; and what we have now to do is to look at the process of that development, and so survey the causes of success and failure in generalisation, broadly and yet with a view to practice.

This way of using the term "generalisation" is indeed only one instance of a method we are about to apply more widely. The peculiar feature of the following treatment is the attempt to use certain well-known and convenient distinctions less sharply and abstractly than is commonly done; in other words, to keep constantly before us the difficulty of answering the question how they are to be *applied*. The general truth of which this method is a particular application (but which I need not here<sup>1</sup> stop to justify) is that all the lines that language

<sup>1</sup> In my recent book on *Distinction* a sufficient justification of it is given. The use here made of the principle may serve as further illustration of the views there reached.

draws are artificial,—that all differences in nature are gradual, and therefore fail to correspond more than roughly to the sharp-cut distinctions we make and use. But along with this principle, which at first sight looks so destructive of clear thought, another fact of an opposite tendency has also to be admitted. In saying that all our lines are artificial, we must be careful to remember that artifice implies *purpose*; the lines are wanted, else they would never be drawn. The interesting question is, What purpose does a given distinction serve, and how far (or on what sort of occasions) does its artificiality prevent its fulfilling that purpose?

There is one class of generalisations with which we shall not here be concerned at all,—namely those which no one ever really doubts when once their meaning is seen. Though doubts may be raised as to which exactly these are, there are possibly some about which we all agree; or at any rate there is no harm in our admitting this

possibility. If any such undeniable truths exist, they lie entirely outside our present inquiry, which is only concerned with generalisations in so far as they can be recognised as doubtful. And of these, "merely empirical" generalisations lie at one end of the scale, and "scientific" generalisations at the other.

The purpose of this latter distinction is clear enough. The more nearly a generalisation approaches the type of the "merely empirical," the less trustworthy it is; to call a disputable generalisation "scientific" is to say the best we can for it. In idea, the two kinds are as far asunder as good and evil; it is in practice—in the application of the names to actual cases—that the two classes (like good and evil) are apt to get entangled. And it is, of course, in practice that we want to discriminate them as clearly as we can.

In idea, again, the nature of the contrast can be still more definitely stated. By general consent, the type of weak (or

“merely empirical”) generalisation is that which Bacon called “induction by simple enumeration.” Two things or events—say a comet and a war—have been observed together (or in succession) once, or several or many times, and without further inquiry we conclude that they belong together in the regular course of Nature. There is a deep resemblance between this familiar mode of argument and that which is sometimes called the fallacy of “*post hoc, ergo propter hoc*,”—where merely because an event, Y, happened *after* another event, X, we conclude that X was the cause of Y; for instance, I took a certain medicine and got well, therefore the medicine worked the cure. The chief difference is that in the former mode (simple enumeration) we make a more definite pretence of *generalising* than in the latter. In the latter we are professedly speaking only of actual (or “concrete”) events, and we may be quite aware that we have still to search for the specially effective circumstances of the

occurrence ; even if the judgment be correct the really effective circumstance may, for instance, have been only one among many ingredients in the medicine. But in both modes of argument if our conclusion is faulty it is so for the same reason,—we have confused the unimportant circumstances of the case with the important ones ; in technical language, we have confused some of the “accidents” with the “essence.” The connection between comets and wars may be an accidental one ; the medicine may have been an unimportant circumstance in the recovery. That is the sole fallacy to which the generalising process is liable, and the two forms just mentioned (“simple enumeration” and “post hoc”) are heads to which all minor varieties of false generalisation may easily be reduced.

But it is when we look at the other end of the scale—“scientific” generalisations—that we get a still more definite idea of the meaning of the contrast. Among generalisa-

tions that have any taint of disputability at all, the highest type is what we call a "causal law." Any generalisation grows better, safer, more guarded against exceptions, as we learn more about the causal aspect of the connection,—as we learn *why* this and that have occurred together (or in succession); what links of causation come between them; how the connection is *conditioned*. If we think of any supposed universal rule which rests "merely" on uncontradicted experience, we see that it is always theoretically possible—and often very practically certain—that the experience is not wide enough to form a safe guarantee. The appearance of universality may be due precisely to the limitations of the experience,—as where a child generalises about the manners and customs of older people. Even our best empirical generalisations are liable to this defect in some degree; we do not know why the stature or the life of man should remain for ever within the limits that



have hitherto been observed, and little more than a century ago we knew no reason against sending news round the world in a moment—except that “experience” seemed to point against its possibility. In all such cases the only remedy we know of is to inquire into the causes concerned. To discuss what this exactly means is one of the main objects of the following chapters.

It is one thing, then, to get the distinction between good and bad generalisation sharply drawn *in idea*, and quite another thing to apply the ideas in practice. When we come to look at actual generalisations it is by no means always easy to class them unmistakably on either side of the line. Any generalisation is somewhat unsafe so far as it depends upon experience, since even our completest experience is limited in its reach; and on the other hand it is difficult to find any single case of generalisation which is entirely innocent of reference to causation. What we find is that our generalisations go

through a gradual progress ; they begin by being (roughly speaking at least) “merely empirical,” and they end by being to a great extent explained as the action of cause and effect. Somewhere towards the lower end of the scale lie, for instance, our generalisations about the weather, about national characteristics, or about ebb and flow in trade or in political opinion. Such generalisations are not wholly unaided by causal theory, but the aid it gives is small. Loosely speaking they may be called merely empirical, and yet as our knowledge grows they are gradually losing their empirical character and becoming more scientific. We have now acquired some causal knowledge of the habits and movements of certain epidemics which formerly seemed capricious, while others seem almost capricious still. Often, even in our looser generalisations, we have some vague idea how the connection may depend on causes ; but so far as the generalisations are loose and doubtful, there is

always room for a clearer insight into the causes concerned, and it is always to such increased clearness of vision that we look for removal or justification of the doubt.

In proportion as we begin to understand the causes concerned, our generalisations get narrowed, and so guarded against attack. The broad rule we first assume is gradually seen to have exceptions; it is therefore limited so as to allow for these exceptions, and to prevent their playing the part of contradictory facts. We begin to see the broad generalisation as only true *under certain conditions*,—its truth as conditioned by this and that circumstance. It was formerly supposed, for instance, that heavy bodies always fall more rapidly than light ones. There is a great deal of plausibility in this “empirical” generalisation, as any one’s observation may show him. An avalanche falls quicker than a parachute, hailstones quicker than snow, snow quicker than dust. Yet the generalisation is now known to be

untrue,—to be true only on condition that the lighter body is (as it often is) more buoyed up by the air. We have learnt that it is not relative *weight* which is essential to relative pace in falling, but simply the resistance encountered ; in a vacuum the speed of falling is the same for everything.

Rough and abstract though the distinction is between empirical and scientific generalisations, we shall here be able to make some use of it. There is another somewhat similar distinction, however, still more widely received and equally rough and abstract, which we had better not try to use at all. That is the distinction between *proved* and *unproved* generalisations. There are few notions more misleading than that any conceivable logic can provide an applicable test of the truth of generalisations, so as to enable us to say of some that they are “logically proved” while others are not. Excluding (as above) those which demand no proof, actual generalisations are *more* or *less* proved,

from their first formation onwards to their extreme development; they are theories, from first to last, and of no theory do we ever know for certain that it has reached its final and perfect form. Further facts may always be discovered which shall compel us to modify even the best of our accepted views of natural science, or the old facts may come to be seen in a clearer light. To claim to have "fully established" any piece of theory is either merely a loose and convenient way of speaking, or else an unnecessary pretence. Practical, working certainty is what we want, and this we often get,—until the exceptional facts arrive which break it down and put some completer practical certainty in the place of it.

On the other hand, the distinction between *criticised* and *uncriticised* generalisations is one that we shall be able to use in the same sort of way as that between empirical and scientific. Though the true beginnings of criticism may in fact be undiscoverable, and

though the perfect end is never reached, the notion of criticising (or doubting) our generalisations is needed at every step in the upward progress.<sup>1</sup> In the next two chapters we shall try to see the manner in which the criticism of generalisations goes hand in hand with their formation, and how observation of fact is interwoven throughout with both these processes.

<sup>1</sup> The notion of *proof* would do as well, if it could be kept to its older meaning of *testing*. But this seems too much to expect. By "proving," nearly every one now means establishing.

## CHAPTER VIII

### OBSERVATION AND GENERALISATION

OBSERVATION of fact, generalisation from facts observed, and the criticism of generalisations, are not (except ideally) three distinct stages ; in actual inquiries the three operations are mingled. As far as we can tell, not even the simplest actual observation is free from the generalising impulse, and not even the weakest actual generalisation is altogether uncriticised. At any rate all ordinary observation is evidently full of generalisation, and all ordinary generalisation is full of reflection and criticism. There is no real difficulty—no difficulty that is more than merely verbal—in admitting the fact of this entanglement. There are many familiar

analogies in the light of which we may regard it. One of the most familiar is the relation between a panic and its causes, or between any two things that become (as we sometimes loosely say) alternately cause and effect. Whatever grows is to some extent in this condition ; what it now is, is partly the effect of what it was a moment ago. The acceleration of the pace of a falling stone is a case in point ; the pace of falling is, when the fall has begun, partly the effect of the previous pace itself. So with a panic ; if it begins with mere uneasiness, still that stirs up an uneasier kind of uneasiness, and so on until we think fit to give it the name of a panic.

Just so it is with observation, generalisation, and the criticism of generalisations. The true beginning of the process we cannot find ; it is much more deeply hidden than even the true beginning of a panic. As soon as we can observe the process at all we find the three factors growing side by side and forwarding each other's growth. Ob-



servation, for instance, is better or worse ; the difference depends greatly upon the selection of what to observe,—that is to say, upon the more or less correct discrimination of important and unimportant details ; and this discrimination is itself already a judgment about the “essence,” and so is dependent upon a stage of generalisation. Similarly, it is hard to find any generalisation which has not already undergone some criticism. We may, no doubt, propose an entirely uncriticised generalisation *as a question*,—we may ask, for instance, whether a given event, A, has any universal connection with another event, B, while our minds remain purely neutral. But we cannot give the answer “yes” to such a question without having to some extent considered the possibility of giving the answer “no.” In deciding to accept the generalisation we have already begun to reason (or criticise), and may become, if we choose, increasingly conscious of our reasons.

Let us look first at observation. This, as already said, is partly dependent on past generalisations, and partly motivated by the wish to support or destroy or improve some piece of generalisation which is taken as questionable, or hardly formed as yet. No one supposes that we ever observe the whole of what is passing. We only observe a selected portion, namely, what we happen to think worth observing; and our selection is always partly determined by our expectation of what the result of the observation will be. When a wrong selection is made, we call this expectation "bias" or "prejudice"; when the results are good we call it "scientific imagination," or "the foresight of genius," or some such complimentary name. In itself, and considered apart from the mode of its application, it is neither good nor bad, but simply a necessary condition under which the work of observation has to be carried on.

Our tendency to select for observation the details which support our existing theories is

so common that it hardly needs illustration. But that a wrong selection may be made even where our object is to attack a theory, the following example<sup>1</sup> will help to show. When Pasteur was investigating the causes of splenic fever, he adopted, very early in the inquiry, the theory of Dr. Davaine, that the disease was due to the presence of a certain parasite in the blood, and that consequently the same disease, showing the presence of the same parasite, could be communicated to other animals by inoculation. On the other side, two professors to whom the theory did not commend itself brought forward, as a triumphant refutation of it, what seemed at first a plainly contradictory fact. They had inoculated some rabbits with the blood of an animal which had died of splenic fever, and though the rabbits had died very rapidly no trace of the expected parasite had been found in them either before or after their death.

<sup>1</sup> Taken from the *Life of Pasteur*, but not further verified. It is at least a characteristic story, and we may hope it is true.

Moreover their blood again had been used to inoculate other rabbits, and these too had died in the same rapid manner, but with the same disregard of what the theory further required. Davaine at once disputed the *fact*. That is to say, he insisted that the two professors must have used blood which was not properly infected with splenic fever, but with some other disease. The professors, however, were equally certain of their facts; they had got their materials from the best available source, namely, from the director of an establishment where numerous animals which had died of splenic fever were constantly brought. But in order to convince the stubborn theorist they tried the experiment again, this time obtaining their materials from the most experienced veterinary surgeon in the neighbourhood. Exactly the same result followed, and the facts certainly here appeared to be too strong for the theory.

It was some years later when the real weakness of the facts themselves came to

light. Davaine's theory had meanwhile been enlarged and improved by the discovery that, if the blood used for inoculation has already begun to putrefy, the animals inoculated will die by a form of blood-poisoning, quicker in its operation than splenic fever, and too quick to allow the true splenic fever parasites time to multiply. This suggested a new inquiry into the professors' experiments, and it was found that the blood used by them, although certainly taken from cases of splenic fever, had not been sufficiently fresh. So that the fact on which they had relied as contradicting the theory turned out to be wrongly—*i.e.* incompletely—described. Through merely overlooking the detail that the animals whose blood they used had been dead some twenty-four hours, their description of it as "splenic fever blood" became essentially false.

Observation of fact, then, depends upon generalisation to this extent, that all observation implies selection (conscious or otherwise) among a crowd of facts competing for our

notice. Certain details we think (or feel) to be more worth observing than others, and the decision as to which these are depends on the state of our theories at the time. But the opposite dependence—that of generalisation upon observation—is even more easily recognised. It is for the sake of generalisation that we observe at all, and the very act of observing *intelligently* is nothing else than the act of generalising from what we observe. Purely unintelligent observation we need not here consider, since practically no human being is acquainted with it, in his own remembered experience. The phrase is either a verbal exaggeration meaning the *less* intelligent kinds of observation, or else is the name of a process of sightless seeing which we may well hesitate to call observation at all. When a man is awake, and in his normal condition, his intelligent, or generalising, observation is more or less at work upon the passing show continually. The rest is memory, or reverie, or a dull and

passive mental condition which is only evidenced to us by the vacant lapse of time. Our active mental life is full of generalisation,—that is to say, of the attempt at *explanation* of things that occur. No matter how restricted our range of ideas, or our chances of observation, the attempt to explain occurrences is at the root of all our alertness of mind in observing, since it puts a premium on correctness of observation. If we live in a village and our minds are chiefly occupied with gossip, this only means that we watch our neighbours' proceedings and *interpret* them according to our lights. Why do we look at a picture, or read a novel? Different people will observe different things in them, but all for the sake of explanation. A painter will perhaps chiefly observe the *technique* of a picture; that is to say, the mode in which the artist has obtained his effects,—that is to say, the explanation of those effects. Others, again, will look for signs that the artist has seen what they would call the

realities of the subject painted. Those who are quick or imaginative will at once perceive generalisation—explanation of fact—everywhere at work in it. Those who are “matter-of-fact” will do precisely the same, only on a more restricted scale. Take the most matter-of-fact observer of the most matter-of-fact picture—say, a portrait—and his judgment that it is like or unlike the original contains at least some attempt to distinguish between what is characteristic of the original and what is not; and to regard anything as having such and such characteristics is to generalise about its character. The expression of a face is subject to alterations, and to recognise a painted and fixed expression as characteristic is to see behind the alterations—to have some glimpse of an “essence” which underlies them all. In novel-reading, again, the same motive is even more evident. Whether we read merely “for the story” or with any other purpose, it is always the detailed picture of reality that is



interesting, and interesting precisely because of the juxtaposition of the details; there must be, to our view, some *reason* for such juxtaposition, or else we lose our interest. And to see a reason for juxtaposition of details is to generalise about what facts belong together in Nature. Though we read a story for pleasure and not for profit, yet no one takes pleasure in a story that seems written by a bad observer of facts. Though an improbable farce may amuse us, its improbability must seem intentional, not the result of ignorance. Absurdity, like all forms of paradox, must find a footing in that which is not absurd, and will be more acceptable fiction if it contains something "truer than fact." A Zola and a Dickens may differ widely in method and in subject-matter, and yet not in observing power.

Owing, however, to the rapidity and unconsciousness of most of our commoner daily explanations of fact, the process is not very easily analysed in them. But there is no

need for us here to make the effort. Quite apart from these hasty and trivial kinds of observation, life presents to every one plenty of opportunities of inquiring seriously and deliberately into the ways of Nature. The most prominent of such occasions are perhaps those that we vaguely describe as "scientific" problems. All scientific inquiries, from the deepest researches of the specialist down to those which a layman can easily follow, are alike in the attempt they are ever making to *interpret* facts observed. That is the sum and substance of scientific work, and the wide generalisations sometimes reached are not the work itself but rather the work's reward. In every science a number of such express generalisations, better or worse in quality, are already attained; and while scientific instruction may largely consist in teaching these as true, scientific inquiry proceeds on the assumption that there is still room for testing and refining them by wider and more careful observation of facts. The mental

alertness of the extreme scientific specialist only differs from that of the novel-reader, or the village gossip, in the degree in which by means of care and previous knowledge he guards his results against precisely the same occasions of error.

That all intelligent observation contains an attempt to *explain* the observed phenomena, and that all explanation involves generalisation, can be seen in another way. We may ask what are the needs which observation tries to satisfy ; what sort of problems does it set out to solve ? Sometimes, it is clear, the observer is directly seeking to form a generalisation, as when Darwin spent eleven years in observing the effects of the cross-fertilisation of seeds ; or to improve an already accepted generalisation by means of accurate measurements, as is continually the aim of the closer inquiries of Science. In such cases there is no difficulty in seeing that the object desired is an explanation (or a more careful and detailed explanation) of the

facts. Darwin relates how he noticed accidentally, two years in succession, that some crossed seedlings were stronger and better-grown than some self-fertilised ones from the same plant. He wanted to know *why* this was,—what general law, or laws, the fact so observed comes under.

Sometimes, again, the purpose is to find the unknown cause of an event which has happened,—as in all detective inquiries, whether the event be a crime or any other occurrence in Nature. We observe the details of the event, in order to learn from them exactly how it happened. Here again the object is to explain the occurrence, and—as we have noticed already in speaking of circumstantial evidence<sup>1</sup>—every detail observed gets its meaning through generalisation. Any detail is, to us, unimportant except so far as we see some hope of putting a meaning upon it. Here also, therefore, intelligent observation of fact is nothing else

<sup>1</sup> See p. 41.

than generalising observation. In this respect it does not make any difference whether our question be the definite one, "*Who* did the deed?" or the vaguer one, "*Why* (or *how*) did the thing happen?"

Lastly, we sometimes merely want to know *what* an observed thing is. There are two familiar forms which this inquiry takes, and one of them is only quite roughly distinguishable from that last mentioned. In asking, for instance, "What is that?" (say, a rustling in the bushes) we are seeking an unknown cause; and wherever the question, "What is it?" is rather a question of fact than of correct naming, the same is true. The question "What is it?" may thus mean either, "What is the cause of that appearance?" (e.g. "Who is that man in the distance?") or "What is the class-name that rightly belongs to it?"—as, for instance, when some unfamiliar flower or bird is seen. In the latter case, it is a question of finding the *genus* and *species*,

and so of seeing analogy, and so of generalisation.<sup>1</sup>

There is yet another and simpler way in which we may look at the matter. All observation of fact is reducible under two heads,—observation of *things* and observation of *occurrences*. The division is merely a convenient one; that is to say, every observation belongs to one or both of these heads. Since nothing is absolutely permanent we can seldom observe a thing without also observing some changes that occur in it. But whether we observe specially the changing details, or the relatively changeless ones, or both, there are only two questions we ever want to answer about them,—how they came to be as they are, and what is coming next? In short, the one aim of all inquiry into fact is *explanation*, and the practical purpose of explanation is *prediction*; in observing anything that exists or occurs, our first object is to understand how it came to

<sup>1</sup> See pp. 33-40.

be (or to occur) as it is, and our remoter object is to use the knowledge for the purpose of looking ahead, and so managing that fraction of the universe over which we can get control.

## CHAPTER IX

### GENERALISATION AND CRITICISM

IN its less indefinite forms, where we can notice better what is being done, generalisation is the recognition of a tie which binds *two* facts together as belonging to each other by natural law. There is only one aim in all generalisation—the finding of *signs* that are fit to be trusted, so that given one fact, another may be inferred. Hence the question whether a generalisation is true may always be stated as the question how two facts are connected in Nature,—whether quite loosely and “accidentally” (like comets and wars), so that we cannot safely use the one as pointing to the other, or with what degree of closeness of connection. Evidently



the kinds of connection that exist are very various and complicated. The different degrees of trustworthiness in our generalisations are, for practical purposes, infinite. But in every generalisation, good or bad, *two* facts are held in view, and the problem is to discover exactly what is the tie between them.

Our means of doing this, and of criticising the discoveries as we make them, consist entirely in observing the facts as they occur. There is nothing else to be done. It is not as if we had a choice between two methods, one of which did not consist in observation but in something wholly different,—say guesswork, or obedience to authority. These latter methods also depend on observations. Guessing never takes place absolutely at random ; it is always suggested by something observed. A guess may be good or bad, but a bad guess is only a bad use of observations. Similarly, it may be right or wrong in a given case to rely on authority, but what

we are then doing is accepting at second-hand the results of observation, which may have been well or badly performed. There is never any generalisation made, or criticised, except as a result of the observation of facts.

The aim of the criticism of generalisations is, as we noticed in Chapter VII., their reduction from the "merely empirical" type to that of the "causal law." When two things or events have been found occurring together or in succession, the question arises how far the connection is universal and necessary. Is it independent of other conditions, or on what conditions is it dependent, or is it accidental? The better or worse solution of the problem evidently resolves itself into a greater or less acquaintance with the conditions under which the things or events occur. If we knew nothing at all of these conditions, any generalisation on the subject would be merely empirical.

But in practice this is never quite the case. As far back as the individual can

remember, he has always had some knowledge of causes and effects, some knowledge of the conditions under which things happen. The same is true of the most primitive men of whose minds we can form any notion. From earliest childhood and from pre-historic times, our actual theories as to the manner in which things or events are connected are always guided by analogy with other better-known connections, and checked by a sense of what is possible in Nature. We come to no inquiry with a perfectly candid mind. Indeed, since a perfectly candid mind would be a blank and unintelligent mind, there is small need to wonder that our memory and imagination are silent about what exactly took place when our minds were in that condition. As far as we can observe the actual workings of our minds, they reason (more or less) before accepting any piece of theory; acceptance, even in its beginning, means at the same time rejection of something else, for reasons that may be more or less discovered.

In order to picture our reasoning power at work upon the pieces of generalisation which are always crowding forward for acceptance, it is important to remember that we never, in any circumstances, have only a single piece of generalisation in view. It is always a choice that is offered us. Convenient as it may be to speak of acceptance or rejection—as if the latter were purely negative—it is also a little misleading. Rejection of a definite theory may sometimes mean acceptance of a less definite one, or the recognition of several alternatives among which our choice is distracted; but a mind which is seeking an explanation of a fact observed is never wholly innocent of theory as to the nature and meaning of that fact. This truth is only another aspect of what has just been noticed,—that we can never observe any fact without finding *something* familiar in it, something whose nature and ways are not entirely unknown. The more puzzling details are mingled with less

puzzling ones, and the problem is to increase the relative number of the latter. But, few or many, there are always enough to give us the beginnings of a theory. It is just as much a theory when we hold that of two given facts one "had nothing to do with the other," as when we believe them to be ever so closely connected. They came together *somehow* on the occasion, or occasions, when they were found so, and if we think the connection "accidental" we must have reasons for thinking so, just as much as for any other view of its nature. To inquire into these reasons is to criticise the theory.

Though our choice among the various possible ways of explaining how two facts have occurred together (or in succession) is probably never quite independent of reasons, there are wide differences of quality between the explanations we actually reach. "Mere guesswork" (so-called) lies at one end of the scale, and scientific theorising at the other. The difference depends on the extent to

which our first guesses are checked and corrected by close, intelligent observation of the facts. When this testing of guesses is very rapidly and unconsciously done we call it "genius" if the problem be a difficult one, or "common sense" if it be fairly easy; but we are here concerned rather with the slower and more conscious method of improving our generalisations,—a method which no genius, and no common sense, can at all times do without.

The key to this method is *analysis* of the facts observed,—a process which we all understand and use to some extent in the commonest affairs of life. There are few mental habits more deeply ingrained in every one than that of regarding an observed occurrence as made up of many smaller occurrences, and any thing or quality as divisible into parts or factors. From large events like the French Revolution down to small events like pulling a trigger, that is the way in which we always try to under-

stand them. And though there are many occasions when, for a passing purpose, we think of an occurrence as undivided, and though some occurrences are so brief that our attempts to analyse them are baffled, yet we never doubt that all occurrences are, in themselves, analysable ; so that when we fail to find their component parts we admit that the failure is due to the limitations of our own perceptive power. The same is true of "things." Whatever has extension in space or time is regarded as being divisible ; we suppose that even the smallest actual fraction of matter has parts though we cannot see them.

It is in this way that we are able to make use of the less puzzling details of an occurrence, so as to help forward the explanation of the occurrence as a whole. The manner in which the operation takes place is so obscure when discussed in general terms like X and Y, that our best plan will be to begin by fixing attention upon the plainest possible

examples, and seek to rise from them to a wider view. For this purpose the kind of example we most require is common enough. Any generalisation, no matter how trivial, will serve for illustration if only its truth be doubtful. And it is better to keep in view not questions which may have seemed doubtful to primitive man, or to very young children—like the question whether “fire burns” or not—but rather those that an ordinary grown-up person will easily take as doubtful. For with all our best endeavours, it is hard to leave the point of view of the average man.

The facts whose mode of connection we want to discover are either simultaneous or successive; we may want to know, for instance, whether two qualities—say red hair and a quick temper—are connected otherwise than accidentally; or, on the other hand, whether two occurrences, with an interval between them—say a red sky at night and a fine day to follow—are so connected. But



since in both kinds of inquiry we are dissatisfied with a "merely empirical" solution for the same reason,—viz. that the next case we meet may be an exception to the rule—the difference does not affect the point we are now considering. In both cases a causal view of the matter is what we want to find, if possible, and this we may get even in the case of simultaneous phenomena; for we can sometimes trace them to a common origin,—as the ticking of a clock and the movement of its hands may both be traced to the spring. Rightly or wrongly, our ordinary notion is that Cause and Effect are successive, and not simultaneous,<sup>1</sup> and therefore even where we are seeking the connection between two simultaneous phenomena, the attempt to make our view a causal one involves our finding successive phenomena that shall account for them. So that, in both sorts of

<sup>1</sup> Where they continue and overlap,—*e.g.* sunlight and its effects,—still we commonly assume that each portion of the cause precedes its own special portion of the effect. But see also pp. 139-145.

inquiry alike, as soon as our judgment ceases to be "merely empirical," the process resolves itself into finding actual occurrences one of which comes after the other, and then making the leap (with what caution we may) from *post hoc* to *propter hoc*. That is the first recognisable step in forming any piece of generalisation critically; and the beginnings of it are seldom or never absent from our most empirical generalisations. There is hardly any quality—not even weight, or colour, or chemical constitution, or life—about whose mode of production, or of increase, our minds are absolutely blank. When we speak of things being "created" we mean that we do not know how they came to be as they are; but we never doubt that their present amount or condition is the result of some process of causation, and we possess a growing mass of knowledge which helps us gradually towards a fuller view of the secret ways of Nature. The doctrine that such and such things were somehow suddenly

formed is in many directions being forced to give way to a more definite view of their origin as gradual. Within the last half century great steps have been made in our knowledge of origins ; we are now no longer content to think of the hills, or the rocks, or biological species, as suddenly formed, and we can even speculate a little about the origin of planets and suns.

In everyday life, as contrasted with science, we are mostly content to leave our generalisations somewhat vague, or at any rate not to seek for a perfect *expression* of them. If we can get a good working rule, whose exceptions are not obtrusive, that satisfies our common needs ; and when the uncommon occasion comes we rise to it more or less successfully. Still, we often take an interest in knowing as exactly as possible how certain things are caused,—say health, or profit, or pleasure, under certain conditions. It is only an accident that we do not in these cases care to make a formal generalisation, or to state a

law of Nature such as might be desired in medical science, or economics, or psychology. The inquiry into the facts is carried on in precisely the same way whatever be the ultimate form into which we want to throw our knowledge ; it is a search for cause and effect in the one case as in the other.

One of the most empirical of such kinds of inquiry takes place where a layman (in the medical sense) experiments upon himself with patent medicines. He suffers from sleeplessness, let us suppose, and casts about for means of curing it. Imagine him to have tried, say, "Beauchamp's Syrup" on three occasions, each time with some apparent success.

He may possess medical ignorance in a high degree, and yet he will have some material for the beginnings of a theory about the connection between the two facts—the taking of the medicine and the alleviation of his trouble. If he be extremely careless he may rest satisfied with *post hoc ergo propter*

*hoc*, but if he cares to go more closely into the matter he must analyse the facts. Before and after the taking of the medicine, on each occasion, there were other details some of which his previous experience will tell him may have been relevant,—his mental condition and his dinner, to go no further. It is possible that the cure had nothing to do with the medicine, or was even hindered by it; or again the medicine may have slightly helped a process otherwise begun. The reasoned acceptance of the theory that the medicine worked the cure implies the reasoned rejection of all conflicting theories. Finally, if he decides that the two occurrences were cause and effect, there is still room for making the generalisation more scientific. The medicine itself is an analysable fact, and so is the cure. Some one ingredient may have been the specially effective detail, and there is room for great variety in the details of the effect. There may, for instance, be a more or less rapid waning of the influence of

the drug as the patient gets accustomed to its use. All through the inquiry he will be helped by what he knows already of the effect of that drug, of other drugs, and other influences, upon the human body generally and upon his own peculiar constitution.

In practice the layman does not push such an inquiry very far, but rather trusts a doctor to do it for him. It is easy, however, to think of cases where our own judgment has to be trusted, and where, at the same time, there are strong inducements to form an opinion carefully. The patentee of the syrup, for instance, has to consider the question how he shall best advertise it, and so has to study the laws of connection between money spent in advertisements and money returned in sales. It is highly important to him to cut down to its lowest limit the waste that accompanies all advertising. He knows that advertisements *per se* do not bring in money, but only those that are well devised. He wants to know, therefore,

how to devise an advertisement so as to get a maximum of publicity and attractiveness at a minimum of expense.

Here again, the dullest and least experienced of us starts with a certain amount of relevant knowledge, and may increase it by analysing some of the many facts that cry aloud for his notice. No one can begin, nowadays, to consider the best way of putting out an advertisement without having plenty of facts in mind,—plenty of cases, though incompletely observed and remembered, where other advertisements have been more or less attractive. Try as he may to invent some wholly novel method, he cannot get free from his knowledge of methods already in operation. Indeed, the very notion that novelty is important is itself partly dependent on facts observed; some of the advertisements that have caught his own attention most easily have done so just because of their freshness; he has analysed these observations so far as to

find novelty highly *effective*; novelty, he sees, forms a large part of the essence of the connection between advertisement and return. Thus analysis, if well directed by means of pre-existing knowledge, makes us understand the facts and so get nearer to a vision of their essence. It is in this way that, on occasion, an ounce of theory may be worth a ton of "fact."

There is strictly no end to the complications that may be found in the laws of advantageous advertising, by those who care to discover them; and the only limit to the value of the search is that imposed—as in so many other directions—by the value of time. It may be more economical to put out two moderately good advertisements in the course of a year than to spend the time in devising a rather better one. Many of the complications are vaguely known to everybody, as, for instance, that articles of middling quality repay advertisement better than either the worthless or the excellent;



or again that novelty itself may be pushed too far,—has to be nicely adjusted to the capacities of the public mind, so as to stimulate attention and yet not seem absurd ; perhaps, for instance, some of the far-fetched or misspelt names that vendors give their wares are not on the whole attractive. The further the inquiry is pushed, the more we see that the essence of the connection is something so fine that we can never quite lay hold of it, only approach it little by little as our experience ripens in the light of our growing knowledge of the regular ways of Nature.

These familiar examples are not put forward as being in any way more typical than others that might have been taken. Any example would serve almost equally well for our purpose. Just as, in order to illustrate the whole class of triangles we might draw any triangle at random, so with the process of generalising from facts observed. Any other example would differ

from these only in the degree of some one or more qualities or features, as these two differ from each other in (*e.g.*) the extent to which pre-existing theory is likely to control our observation of fact. Every feature we can trace in any one attempt to generalise is reproduced more or less in all; any one such inquiry is, in regard to each of its features, somewhere on a scale which stretches upward and downward without assignable limit. In trying to understand the process generally, therefore, we must seek to dismiss from our view these accidental variations, fixing attention only on what is common to them all.

For instance, two features that vary considerably may be at once set aside as due rather to difference in the persons inquiring than in the kind of inquiry. These are, first, the extent of the influence of pre-existing theory; and, secondly, the importance of the generalisation when formed. If in one case we give less weight to brute experience than

in another, this is not a necessary result of a difference in the kind of inquiry, but depends on a number of accidental circumstances. Any one may pay too much or too little attention to "facts," in any sort of inquiry, and may come to any inquiry well or ill-provided with previous knowledge.

Again, as regards the importance of this or that generalisation. No doubt, inquiries into the ways of Nature do differ in importance, but importance itself varies with the purpose in view ; and recognition of importance, even for any one purpose, varies from man to man. It seems better, therefore, here to take a more catholic view of what constitutes importance, and to regard all general truths in Nature as equally important in so far as they are true. Even if we personally do not suffer from sleeplessness, and have nothing to advertise, we may admit the importance of these inquiries to the world at large. And the same with astronomical speculations, or experiments in thought-transference, or

the discovery of the habits of fungi in a forest. At any rate no one of us can decide for all other people how truths shall be ranged on the scale of general importance.

Our scientific and our unscientific inquiries into the ways of Nature differ from each other only in the amount of care that is taken with them. In extent, or ambition, neither kind has any advantage over the other, since no portion of Nature is too small for scientific interest, or too large for the looser operation of guesswork. In their aims, and in the kind of risks they run, they are precisely alike. It is only by reference to generalisations that we can ever explain the events we observe ; and our explanations are better or worse, less or more superficial, according to the amount of causal insight they contain. So that the "explanations" we actually reach range all the way from those that depend upon some mere glimpse of connection up to those where we have as full a perception as possible of the causes at work. No doubt, if we take

a high standard of what is required for "explanation" we may hesitate to apply the name where we are conscious of our ignorance of the causes ; still we do not deny that even the weakest generalisation is, as far as it goes, of the nature of explanation,—we only deny that it goes far enough to deserve the name. The difference is analogous to that between a child and a man ; our empirical generalisations are a partly-developed form of what may later grow to be fairly described as insight into causes and therefore satisfactory explanation. Up to this point we have assumed that we all know what we mean when we talk of one thing causing another. But in the next chapter some inquiry must be made into the weak points of this assumption. To a certain extent our ordinary view of causation can only be justified on grounds of convenience, or brevity of expression ; yet we are apt to forget this and so to hide from ourselves a piece of truth whose due recognition may on occasion be of some importance.

## CHAPTER X

### THE NOTION OF A "CAUSE"

IT seems unlikely that the many difficulties which, since earliest times, have perplexed mankind on the subject of Causation should be capable of being suddenly removed. Yet it is sometimes possible to find a way around an obstacle though the obstacle itself remains. The importance of a difficulty depends not on its mere bulk but on its relation to an end in view, and some of the well-known difficulties about Causation are, I think, capable of being circumvented (though not removed) when our purpose is limited as we are here content to limit it. Others, again, require no circumvention, but lie together out of our path.

To the latter kind belong all difficulties which relate to efficiency or energy. However interesting in itself may be the distinction, sometimes drawn, between the *production* of one occurrence by another and a mere *necessity of sequence*, the distinction is of no importance at all so long as our only object is to discover perfect regularity of connection. Regularity cannot be more than perfect, whatever anthropomorphic compliments we are pleased to pay it; and perfect regularity is all we require for inference. If Y follows X with *perfect* regularity, then, no matter whether X produces Y, or something else produces both of them, the connection between X and Y can be used as a perfect ground of inference. The practical difficulty, of course, is to make sure that the supposed regularity is perfect; but this difficulty is not in the least removed by our *calling* a given sequence "causal." For if one man thinks it causal (or perfectly regular) and another man thinks it not so,

which of the two shall we believe? When two disagree on such a point they cannot both be right.

To infer regularity from causality is, surely, to put the cart before the horse. Causal connections are what we want to discover, and our only means of agreeing which they are is to rest our judgment on something of wider value than our mere personal inclination to suppose that we have solved the problem, and that all who happen to disagree with us are wrong. Something more objective than this we certainly have, namely, the observation of fact, where if two observers disagree there are ways of hunting the error down in daylight. The observation of fact is the observation of connections which are more or less regular, and the problem is to distinguish the more from the less regular connections to the best of our ability. The history of the progress of human knowledge shows, with increasing clearness, that better solutions are given in proportion as we



recognise our own liability to be misled by first appearances. It is the testing and verification of theories, not the easy belief that they need no testing, that helps forward our knowledge of the ways of Nature.

There are several reasons why this irrelevant distinction is likely to persist in appearing from time to time in works on Logic. The reason that has least vitality left, perhaps, is our inclination to fancy ourselves infallible. As the world gets older it does get wiser, in this respect,—thanks chiefly to Science, but also to increased means of communication, which tends to break down our provincialism and conceit. A more important reason is the philosophical interest which may legitimately be felt in the question when considered apart from the inquiry into the process of actual human reasoning. It would be very useful to know what "efficiency" really is. In fact, we should then no longer need the slow and patient methods of Science, but our minds would

become a vast encyclopædia, in which the whole course of the Universe, from infinity to infinity, would be unfolded to our view. No wonder that this charming prospect should entice us away from mere matters of detail, and make us somewhat hasty in getting over the ground.

A still more important reason, because more widely felt and saner, is the desire to mark off really causal sequence from sequence which is not yet explained as causal. The difficulty of drawing this distinction is relevant enough, and on the surface it closely resembles the one we have just seen to be irrelevant. We do need to distinguish, as well as we can, between *merely empirical* sequence and causal sequence, or between *apparently* regular and perfectly regular sequence, though not between causal sequence and perfectly regular sequence other than "causal." It is because a merely empirical sequence may not be perfectly regular that we want to mark it off as inferior. It is

an ambiguity in the word "cause" that leads to the confusion between the two inquiries. The irrelevant one involves a piece of theory from which the relevant one is free,—namely, the theory that causation is somehow analogous to the operation of the human will. Whether this theory be true or false, the inevitable effect of holding it is that we mean by causation (or causal sequence) something different from what is meant by those who neither assert nor dispute the theory. To the latter, the phrase "causal sequence" means "perfectly regular sequence"; it is merely a convenient name for the *ideal* of perfect regularity. To the former it means, at most, only one particular kind of perfectly regular sequence.

When people profess to use a word for a mere ideal, and not as implying any theory as to the application of that word in particular cases, the profession is sometimes a pretence or an error, and is still more often suspected of being so. But fortunately its truth always

admits of a simple test. Can we agree to do without the word, and to substitute for it the colourless definition we profess? In the present instance we certainly can do so; it would make some of our expressions clumsy, but that is the only harm. Instead of saying that our problem is to distinguish between merely empirical sequence and causal sequence, we should then say that our problem is to distinguish between imperfect and perfect regularity of sequence. For the sake of clearness of meaning the latter expression is even to be preferred. It confesses openly that the notion of reaching a "causal law" (thus negatively defined) is not *in itself* of any practical service in improving our empirical generalisations, but requires some further knowledge before it can be used,—namely, some knowledge of the manner in which the more regular sequences may be distinguished in practice from those which are less regular. As already noticed, the key to this lies in analysis,

which enables us to use the less puzzling details of a case to throw light upon the more puzzling details. Whatever method might be ideally better, this is in practice the only method available.

The best justification for our negative use of the word "cause" lies in the fact that, after all, no human being, however devoted to metaphysics, has any higher ideal of regularity of sequence than that to which he gives the name of causal. To take an extreme instance of apparent departure from our views, even those who regard the ways of Nature as capricious, or dependent upon the changeable will of the Creator, thereby merely deny that one created thing "causes" another; they *apply* the word "cause" even more restrictedly than those who admit a few cases of "real efficiency" in physical Nature; but they hold it an absolutely universal truth that Nature obeys the will of the Creator, and they accordingly regard that as the only true causation. Most of us, however, do not

now regard Nature as capricious—only as puzzling and insufficiently understood. We now *define* “Laws of Nature” to mean only those uniformities which do not change. We suppose that there are perfectly universal (or regular) connections, and that these are partly known to us, seen as yet incompletely, waiting to be discovered or better conceived. Wherever we think we have found perfect universality of sequence, there we think we have found a case of direct and immediate causation; and if a given sequence, however frequently observed, ever fails to occur on a single occasion, that is proof positive (to all who use the word “cause” in any sense) that such sequence is not a case of direct causation.

Many difficulties are lightened for us as soon as we give up trying to see in causal sequence anything more than perfectly regular sequence, and so trying to explain the latter by the former. It is always

tempting, but always illusory, to fancy the truths of Nature thus dependent upon the names we give them. The assertion that X and Y are cause and effect is a convenient way of saying that we believe the sequence to be perfectly regular ; and the possibility that we are mistaken in this belief has always to be faced. No amount of energy spent in calling them cause and effect will make them any more connected than they really are. No amount of assurance in our beliefs will, by itself, guard them from error.

The difficulties that surround the notion of a "cause," however, are not all of this irrelevant kind. The chief source of those which are really troublesome is the old and ever-recurring puzzle about the continuity of Nature. Try as we may, we cannot conceive of action at a distance, whether in space or time. If the sunlight comes to the earth, *something* must be passed on over every fraction of the journey ; if an event X causes an event Y, the time-interval between them

must be filled by occurrences bridging over the gap. A thing can only act where it is, "but, pray, where is it"? and the question where does an occurrence end and "another" begin, is equally unanswerable except by an artificial distinction drawn to suit our practical purposes. For to understand *how* one occurrence leads to another is always to bridge over the gap between them,—to see it as filled with other occurrences, of the nature of missing links. The farther we carry this process the more we blur the original line of division. Take the two occurrences called "pulling the trigger" and "firing the shot," which is plainly a case of sequence, though the interval is a short one. We all know something about the intervening process, and if we select any part of it—say the fall of the hammer—we are aware of a difficulty in deciding to which of the two occurrences (cause or effect) it properly belongs. The fall of the hammer is, even more directly than the spark which follows, caused by the



pulling of the trigger ; it should therefore be classed as part of the "effect." On the other hand, the fall of the hammer is, even more directly than the pulling of the trigger, the cause of the shot being fired ; it should therefore be classed as part of the "cause."

It is clear that this kind of analysis may be carried much farther. Given any two steps in the series—say spark and explosion—we know that some occurrence comes between them, however short the interval, and that this intermediate occurrence (or some of it) has just as much right to be classed as belonging to the one end of the chain as to the other. It is not properly a chain, in fact, but a stream, or a continuous growth like that between bud and flower ; and we do not talk of the bud as *causing* the flower, but rather as being itself the flower in an imperfect stage of growth. The flower is regarded not exactly as something *other than* the bud, but rather as the bud itself unfolded.

For reasons that will presently be noticed, this example—the firing of a gun—is one where the difficulty is unlikely to be often felt in practice. It is chiefly where the growth is noticeably complex and gradual—as in the case of large political or industrial movements, or mental and physiological changes—that the direct practical effects of the difficulty appear; we are mostly aware that no line, except an arbitrary one, can be drawn between the French Revolution and the “events which led to it,” or between the fears that make us nervous and the nervousness to which the fears themselves are due. But even a simple and well-understood piece of sequence, like the firing of a gun, will serve to illustrate the theoretical difficulty so far as we can recognise that the occurrence is a continuous stream and is only by an artifice regarded as a chain with separate links. Our common notion of Cause and Effect requires that the Cause shall come *first*. But it becomes difficult to regard an event A as

preceding another event B, if we admit that B is properly a part of A itself. The relation of sequence involves the relation of "otherness."

Though the difficulty cannot, I think, be removed, it can be regarded as a mere difficulty of expression, and so circumvented. Some useful hints may be gained by noticing how easily we forget its existence in most of the ordinary affairs of life. One reason for this forgetfulness lies in the fact that our practical purposes are to so large an extent easily satisfied. In ordinary life we take a great number of connections for granted, without ever having occasion to search for the links between them; we leave it to Science to discover what comes between spark and explosion, or between lightning and thunder; we leave it to the gunmaker to know exactly what comes between pulling the trigger and making the hammer fall; the ordinary householder does not inquire closely into the system that comes between meter

and gas bracket, or cistern and taps, or bells and pushes or handles, but treats such pairs of things as "somehow connected," and the connections as trustworthy on the whole. It is only when the connections fail that he feels his ignorance of the process and calls in the aid of the knowing man. Just so it is with all causation; we regard it as *direct* until, either by means of an obvious time-interval or a contradictory instance, our attention is drawn to the missing links. No one seriously denies that the links are there, but our practical needs do not always compel us to explore them.

Another fact that tends to make us overlook the difficulty is the concentration of our interest or attention on a comparatively small part of the total circumstances of the case: Occurrences like spark and explosion have an obvious individuality which saves us all trouble in saying when they begin and end. If a scientific man finds it interesting to inquire what degree of duration in the spark, or of

dryness in the powder, is needed for the cause to produce the effect, still the two occurrences do not, even for him, run together and become indistinguishable; and the same with the plumber and his electric bells. It is only now and then that the line-drawing difficulty takes the form, in practice, of making Cause and Effect seem related like bud and flower. Most events, so far as they are noticeable, occur with a perceptible suddenness which hides their real continuity.

These considerations help us to see that the separation into Cause and Effect is done for a purpose, and that its value depends upon its serving that purpose. Whenever a hitch occurs we are bound to look more closely into the details of the connection. Though it often suits us to speak of *two* occurrences, one coming after the other, yet it may sometimes be useful to remember that these two, taken together, have just as much right to be called a single occurrence as have

each of them separately. The grouping of any occurrence, large or small, around a central point, is conditioned either by our views of what is convenient—as in historical inquiries—or else by those susceptibilities of ours which are only not “views of convenience” because they are so unconscious and so inevitable,—till their shortcomings appear. Our practical ends, our interest, our attention, are all in ourselves as contrasted with “nature.” To this extent the line we draw between antecedent and consequent is an artificial one; they are more or less ill-defined parts of a whole which it suits us to pull asunder.

We shall see, in the next chapter, some of the results of this admission. Meanwhile let us notice that the line between antecedent and consequent is not the only one we have to draw. The limits of the larger occurrence itself are just as artificial. It also is a part of a still larger occurrence which extends beyond it in time and space. To suit our con-

venience (or needs) it is singled out of the continuous course of nature. An immense mass of occurrence, partly contemporaneous and partly not so, is discarded as not properly belonging to it,—as *accidental* to it, or outside its *essence*. There is thus a threefold process of definition performed by us in every causal inquiry, as a starting point: the length of the total occurrence, its breadth, and the line that cuts it in two. Theory as to the "essence" of the occurrence, and of its parts, is at work throughout these operations; and theory always contains a doubtful element. In practice we do not all agree as to what was the true beginning, or the true end, of many an occurrence in history. Still more likely are we to err in selecting what contemporaneous details shall be included as part of "the occurrence." The stretch of time during which an event occurs is full of occurrences which we regard as "other"; and any of these may be more closely connected with our occurrence than we imagine,

—may, in fact, be properly speaking a part of it, and wrongly regarded as “other.” History, for instance, is not well conceived as merely an account of battles, or deeds of kings, though the superficial historian often tends to make it appear so. An event must be “striking” if it is to strike a bad observer’s notice, or to be reckoned by him as really a part of the occurrence on which his attention is fixed; and so long as our observing power remains at all imperfect, so long we must be liable to overlook some of the less salient features of any occurrence, —as we saw that the two professors did in the inquiry into splenic fever infection.<sup>1</sup> It remains now to ask how this element of artifice in our view of the causal relation should affect, in practice, our attempts to get from the more empirical kind of generalisation towards the type of a causal law.

<sup>1</sup> See p. 97.



## CHAPTER XI

### THE SEARCH FOR A CAUSAL EXPLANATION

THE question next before us is as to the practical results of the admissions made in the preceding chapter. How are we to give due weight to them and yet prevent their leading us into a deadlock? In ordinary life our tendency is to disregard them almost altogether; but this can hardly be called a solution of the problem, though it may help to suggest one. Our common practice is to ignore the difficulty until it forces itself upon our attention; can we meet it in any less grudging spirit, and yet not so as to yield to it too far?

To yield to it too far would be to give up speaking of Cause and Effect altogether.

That certainly seems the first and most natural result of admitting that Cause and Effect are one, or that the Effect grows out of the Cause, instead of following it as one occurrence may follow "another." But—supposing we could really preserve this view consistently—it would reduce all causal explanation to a chimera. There would then be only one "occurrence," namely, the *unanalysed* course of Nature itself; and analysis is absolutely required for explanation. To attempt to "explain" anything by merely calling it a part of the course of Nature would be about as useful as to describe the position of anything by saying that it is located in the universe. Something more definite is required, even if we have to make it definite artificially. It is easy to regard direct causation as a chimera; so it is, if every *ideal* is so too. But the notion of its being something worth hunting for, and approachable with increasing success, is one that may nevertheless be of service towards

the practical improvement of our view of the ways of Nature. The distinction between Cause and Effect must be treated just as we have treated so many other distinctions which we have admitted to be abstract and artificial. Our aim must be to seek for the use or value it really has in practice, and at the same time to guard against its being extended beyond that use.

The use of the notion of Cause and Effect is, as already said, to help us to get some insight into the *conditions* under which X may be taken as a sign of Y. X and Y, as they occur in Nature, are always entangled with non-X and non-Y—that is to say, with “other” things or occurrences; and it is always possible, until the contrary is shown, that the essence of the connection between X and Y lies partly in these details that seem perhaps at first so accidental. The question, then, turns upon the importance or unimportance of this, that, and the other detail as regards the connection we are

seeking to establish or refute. It is through what we already know, or suppose we know, about special causes and their effects, that we judge of the importance of any detail; and what we know about causes and their effects is simply what we know about perfect regularity of sequence. Now the notion of sequence involves the notion of "otherness," or difference, since we cannot intelligibly speak of a thing as following itself. Therefore we cannot judge of the importance of any detail unless we make the assumption that it is something distinct both from its own Causes and its own Effects.

We have also noticed that in most cases there is little or no practical difficulty in making the separation. We naturally tend to regard things and events as distinct from each other, since it is their apparent distinctness or suddenness that gives them their apparent individuality. The danger, in fact, is almost entirely on the side of taking our own distinctions too seriously,

—forgetting that they are of human origin, and becoming enslaved to them. To be so enslaved is nothing else than to forget the existence of intermediate links ; what exactly, then, is the harm that can result from such forgetfulness ?

Intermediate links in a chain of causation are so many opportunities for counteraction, in the same way as the length of a piece of railway provides opportunities for an accident. They are intermediate *conditions*. The pull on the trigger will fire the shot if, and only if, the catch, the spring, the hammer, the cap, and so on, all act in the expected manner. Therefore our forgetfulness of intermediate links takes effect just in the same way as our forgetfulness of conditions generally ; it may give us a false security. The action of the causes concerned is always better understood in proportion to the clearness with which the *conditional* character of every piece of supposed causation is recognised. We must constantly take the leap

from *post hoc* to *propter hoc*, and yet in doing so we must remember that we are always making a certain amount of assumption. The difference, then, between the ordinary practice of waiting till we are forced to recognise the intermediate links, and the scientific habit of going out to meet that recognition willingly, resolves itself into a consciousness of making assumptions, and a consequent care in verifying those assumptions by the best available means.

It should now be evident that the effect produced upon our causal explanations by recognising the artificiality of the line between Cause and Effect is of the same nature as that produced by recognising the need of causal explanation itself. Both recognitions involve the admission that a supposed connection has a conditional element in it, or that the working theory that it is unconditional stands always in need of further verification. To recognise that any case of supposed causation, however apparently

sudden and immediate, is really a gradual process, is to recognise an element of doubt as to its perfect regularity, just as we recognise that any empirical generalisation is somewhat uncertain. There is only a difference in degree of subtlety between the two admissions; the one is an extension of the same method that directs the other,—the method of doubt, or reflection, or criticism, which springs from our sense of the incompleteness of any piece of human explanation.

It is not an easy matter to reach any practical general rules for regulating the amount of doubt which a given case requires. So much depends on the exactness which will serve the purpose we have in view. A dabbler in patent medicines, or an appraiser of other people's conduct, or of a novel or a picture, is commonly satisfied with a very moderate exactness of view; and even an advertiser trusts a good deal to chance. It is to scientific inquiries that we must turn if we wish to see the art of exact inquiry at its

best, and among scientific inquiries themselves there are great variations in exactness and subtlety. Sometimes the correction of a gross error is the immediate aim of Science, sometimes the utmost refinement that delicate measuring instruments can provide.

This difference of aim, or standard, among scientific inquiries, is partly conditioned by the state of our knowledge at the time, and that again by the manageableness of the things we have to deal with. In Biology, for instance, we are obliged, on the whole, to be content with less exactness than in Chemistry or Physics. In fact, Biology, even in recent times, provides many examples of first emergence from the "empirical" stage of explanation.

The following case may serve as an example. A few years ago the attention of a scientific specialist<sup>1</sup> was directed to a disease which attacks certain kinds of lily. The usual symptoms of the disease were already fairly well known. First, small

<sup>1</sup> Professor Marshall Ward.



rusty-coloured spots appear on the buds and leaves; gradually these spots grow larger, and, under favourable conditions, — damp weather, for instance, — spread over the whole plant. After a time the tissues of the leaves collapse and rot away, and a peculiar gray mould-like fungus is found growing in them.

Botanists had long been acquainted with this particular fungus (*Botrytis*), growing on beans and various other plants, but since it was “always found” springing from the dead or dying tissues, they assumed that something else had killed the plants, and that the fungus then grew on the decomposed substance of the tissues, — much as ordinary gray mould (*Mucor Mucedo*) lives on dead vegetable remains.

Professor Marshall Ward's observations showed, however, that the *Botrytis* is capable of growing on living tissue, as a parasite. Having sown the spores and cultivated them in the laboratory, he found that the fungus,

if kept well-fed and vigorous, excretes a soluble substance which has the power of softening and swelling the material (*cellulose*) of which the walls of the tissues of plants are composed. The experiment was then tried of sowing the spores in a drop of water on a young growing bud of the lily, and the fungus "was seen to bore its way into the tissues, evidently by excreting the above-named soluble substance, which enables its filaments (*hyphæ*) to penetrate the cell-walls." Once inside, it soon increases in length and thickness, and begins to send out branches. At first the branches run in the cell-walls only, but when the tissues break down owing to the destruction of the walls the parasite spreads in all directions, and soon destroys the plant.

Two theories here compete for acceptance—one, that the *Botrytis* is accidental, the other that it is essential, to the death of the plant by the well-known disease. The former theory was due in part to the supposi-

tion—from previous “observations” — that the Botrytis never grows on living tissue, as a parasite; and the means by which the newer theory was established consisted in a careful review of the facts observed.

The value of the later observations consists not in their quantity, but in their quality. As far as quantity is concerned, the observers who had “found the Botrytis growing only on dead tissue” were numerous enough, and the number of dead specimens seen must have been legion. It was not *number* of observations on which the second theory relied, but on tracing the causation step by step. The excretion is viewed as something made by the fungus; the softening of the walls as caused by the excretion,—and then as essential to the entry of the hyphæ; the further growth of the fungus as facilitated by the entry of the hyphæ; and its final victory as depending upon its previous further growth. At each of these steps, Causes and Effects are supposed to be seen in operation;

and—subject to the question how far they are correctly seen—the explanation is at any rate much fuller and more careful than that which had satisfied the former botanists. For the purpose of correcting an error which was then in possession, the exactness attained was sufficient. Yet there is evidently room left for still fuller explanation, when the need for it shall arise. At each step an assumption is made. The facts observed are merely facts of sequence ;—*after* the growth of the fungus, the excretion is found ; *after* the excretion, the softening of the walls ; and so on. The leap from *post hoc* to *propter hoc*, though carefully made, always involves a risk. In mapping out the continuous process into separate Causes and Effects the intermediate steps are left unseen, the further necessary conditions unnoticed.

From a child's observation of a conjuring trick, up to the most careful work done in a laboratory, the leap we take from *post hoc* to *propter hoc* necessarily involves the neglect

of all details except those we regard as important, and our view of the importance of this or that detail must change with the growth of our knowledge. It is not only "empirical" theories, but causal ones too, that need revision. Mistakes in supposing a given sequence causal are of course very frequent in daily life, but neither are they wholly unknown in careful scientific work. If we make an infusion of hay, "sterilise" it by boiling, seal it hermetically against the entry of air, and then some days later find it swarming with bacteria, we may yet be wrong in concluding that there we have an instance of dead matter *causing* life. Our sterilising process, or our exclusion of germs, may not have been performed with sufficient care.

The search for a causal explanation must, then, be checked throughout by distrust of our own readiness to assume that given sequences are causal. The best observer and generaliser is he who can use the notion of Cause and Effect on the right occasions and

yet not become a slave to it. All through an investigation the inquirer must have the notion of Cause and Effect present to him,—as a guide to further inquiry, not as a final test of results. With the facts of sequence stretched out in a chain before him, he must separate as well as he can those links where the causal connection is less satisfactory from those where it is more so ; not in order to cease for ever from doubting the latter, but in order to turn attention first to the weakest part of the chain. It remains now to ask what can be said in a general way as to the safest sources of our conviction that a given link of sequence is causal.

## CHAPTER XII

### AGREEMENT AND DIFFERENCE: QUANTITY AND QUALITY OF EVIDENCE

It is easy enough to lay down general rules for the weighing of evidence,—just to the same extent and in the same manner as it is easy to lay down rules for the conduct of life. We can make them undeniably true, but only at the expense of their applicability to difficult cases. “Evil communications corrupt good manners,” no doubt, but the true difficulty is to know exactly what communications are evil. “Selfishness” is the essence of sin, but the difficulty is to distinguish clearly between selfishness and self-realisation. “The greatest good of the greatest number” is an unimpeachable but

not an unmistakable aim. Similarly we can lay down excellent Rules of Method, or Inductive Canons, but we can no longer (with Descartes)<sup>1</sup> complacently "take the firm and unwavering resolution never in a single instance to fail in observing them." Any rule for weighing evidence is of necessity in this dilemma: either we can follow it exactly, and it may lead us wrong, or else we cannot know whether we have followed it exactly until we already know that it has not led us wrong. No actual rule, therefore, can be trusted to lead us right. Not in using Mill's "Inductive Canons," for instance, but in readiness to question the results actually attained by attempting to use them, lies the difference between a good and a bad observer and generaliser.

Since Mill's Logic became influential, it is widely known that the attempt to make use of Agreement and Difference, or permanence and change, is the one guiding plan of all

<sup>1</sup> Discourse on Method. Part. II.



experiment and observation, good and bad alike. Sometimes our attention is caught by a resemblance, sometimes by a difference ; where we find resemblance we expect further resemblance, and where we find difference we expect further difference. In that expectation we are perfectly safe, until we try to specify *exactly what* further resemblance or difference we expect. The whole practical difficulty consists, not in believing that "the same cause is always tied to the same effect," or that "difference anywhere implies difference elsewhere," but in seeing exactly what sameness and what difference is to be foretold in the given case. The abstract axioms of Causation, and all abstract rules deduced from them, are undeniable ; it is in their application to special cases that the endless difficulty lies.

The least scientific use is made of Agreement and Difference when we are content to judge of their *amount*, without analysing it into details of greater and less importance. This is done to a great extent

in our rough and rapid use of analogies. We find a "remarkable likeness" or an "immense difference" between two things or events, and without stopping to inquire carefully into the details of the likeness or difference, and their special meaning or importance, we infer that the likeness or difference extends in such and such a direction. As noticed already, careful generalisation consists in the intelligent observation of details, and success depends upon a knowledge of what those details signify. We require, therefore, to substitute for the notion of amount (or degree) of agreement and difference some other notion less easy-going. The more content we are to judge of them without the help of analysis, the further we are from the scientific end of the scale of generalisation.

The word *essential* as applied to resemblance and difference exactly meets this need. When the term essential resemblance or difference is used with care, it may help us to bear in mind the value of analysis.

Like all phrases, it is open to abuse, of course; and like all phrases it can do nothing (by itself) to acquaint us with the special secrets of Nature. In practice we are rather too apt, perhaps, to fling the word "essential" about; it is often employed, with fatal facility, both in supporting and in disputing an analogy. It is easy to claim that a resemblance or a difference is essential; the difficulty is to justify the claim. But, so long as we clearly see what the claim involves, no harm can be done by the use of the word.

When we speak of one thing or event as being essentially like another we never mean *exactly* like; and by essential difference we never mean *entire* difference. Exact likeness and entire difference are, in fact, unknown to us in nature, and the phrases are admitted to be a mere rhetorical exaggeration. But we always mean by "essential" *important*, and so important as to be (for some purpose) sufficient. If two things

or events are essentially alike, that means that their difference may be neglected; and if they are essentially different that means that their resemblance may be neglected. Now neglect of a resemblance or difference which is really there can only be justified by reference to a purpose; its justification depends on the *irrelevance* of the details neglected, in regard to the special conclusion we draw. The points of likeness between the Suez and the Panama Canals, for instance, may be neglected if the points of difference are sufficient to over-ride them on the question whether the newer enterprise will pay. The importance of any resemblance or difference thus always varies with the purpose for which it is used. There is no point of resemblance or difference which is important or unimportant for all purposes at once. This truth is for the most part overlooked in our ordinary habits of thought. A difference or resemblance may often be disregarded "for all practical pur-

poses," which never really means more than for a good many practical purposes ; and so we are apt to think of their importance as a quality capable of a simple test, in the same sort of way as the heat or the size or the weight of objects may be tested. Until we make the notion of purpose—*i.e.* of the argument for which the resemblance or difference is used—the turning-point of the inquiry, we shall never get free from the natural unscientific confusion between essential resemblance or difference and mere degree or amount.

The same meaning attaches also to the word "essential" when we speak of one thing or event being *essential* to another. Essential in this connection means again *important*, and so important as to be necessary. Every thing or occurrence to which we can give a descriptive name is regarded as composed partly of essential details, partly of accidental ones ; some of the circumstances are necessary to "its" existence,—so

that without them it would not be what it is, —while other details may be omitted or changed without affecting its identity. This is equally true of all occurrences independently of their duration, —independently, therefore, of the question whether we regard them as parts (antecedent and consequent) of some larger occurrence, or as themselves composed of parts; and it is equally true of all things whether regarded as attributes or as substances. So that when we speak of an occurrence being “repeated” (or of something being “found on more than one occasion”), it is only the essence of such thing or occurrence that we profess to have in view; its accidents may vary to any extent without detriment. And further, to every occurrence *some* antecedent and consequent are essential, and to every substance *some* attributes.

When we bear these truths in mind it becomes increasingly evident that we cannot use the notion of amount of agreement or

difference except for superficial purposes. In all cases the question really turns upon the *kind* of agreement or difference,—that is to say, upon the relation of the agreeing or differing details to the “X” and “Y” we have in view. If they are essential, the generalisation is sound; if they are accidental they have no weight whatever. The problem how to generalise correctly resolves itself, therefore, into that of excluding from our view the accidental details whether of resemblance or of difference; it may be roughly compared to the task of extracting a precious metal from its ore, by breaking up the ore and sifting away the refuse.

It is thus always something added to the mere facts of Agreement or Difference that is needed to guarantee the use we make of them. In Mill’s account of the Inductive Methods this is referred to by the proviso that the Agreement or Difference must be found to consist in “only one circumstance.” It is true that if we could know when a

circumstance is really single, and not composite, this proviso might be turned to practical account. As things are, however, we not only do not ever know this, but we do know that the apparent simplicity of any circumstance is always an illusion or at least an artificial simplicity made by ourselves for a purpose. The help or hindrance due to the artifice, in any given inquiry, depends on whether the line between "one circumstance" and "more than one" has been drawn in the best manner possible with that special inquiry in view. In other words, it depends on our having rightly sifted out the essential from the accidental circumstances. That is, in all cases, the problem to be solved in generalising; scientific inquiry begins with the question, *What is essential to what* in an observed occurrence? The better we understand an occurrence in its details, the better we shall understand it as a whole.

When we look at the contrast, such as it



is, between the use that is made of observed Agreement and that which is made of observed Difference, we can hardly fail to be struck with its analogy to the contrast between *quantity* and *quality* of evidence. As Mill pointed out, the Method of Agreement is chiefly used where we need a large number of observations, the Method of Difference where we can make a carefully-planned experiment. In the former Method our aim is to reduce the Agreement to "a single circumstance" by getting the instances as various as possible and discarding all the differences as accidental; in the latter Method our aim is to reduce the Difference to "a single circumstance" by getting the instances as nearly alike as possible. It was on this account that Mill gave a decided preference to the Method of Difference, speaking of its "rigorous certainty," while he regarded the Method of Agreement as "an inferior resource, in case the Method of Difference is impracticable."<sup>1</sup>

<sup>1</sup> See also *System of Logic*, III. viii. 3—"It thus appears to be

The contrast between quantity and quality of evidence is a familiar one, and was referred to in our example of the lily disease.<sup>1</sup> We often have to strike a difficult balance between the value of a large number of rather shaky observations and that of a smaller and stronger group. On looking closely, however, we find that the contrast cannot be quite consistently preserved, since so far as quantity has any value it thereby becomes a kind of quality. In a sense, we may say that mere quantity has no value at all; but there is not much practical use in this statement, for *mere* quantity is an abstraction seldom or never actually encountered. By virtue of their individual differences a group of instances is generally more instructive than a single one; and so far as by the Method of Difference alone that we can ever, in the way of direct experience, arrive with certainty at causes. The Method of Agreement leads only to . . . uniformities, which either are not Laws of Causation or in which the question of Causation must for the present remain undecided."

<sup>1</sup> P. 156.

we fail to understand our observations there may be some use in multiplying them, though the multiplication by itself will never make them correct. The practical difficulty (which repetition and multiplication are intended to lighten) is that of knowing how far our observations are mistaken. The more interesting question therefore is, On what sort of occasions, and within what limits, is it useful to repeat experiments or to collect a large number of more or less random observations?

It is easy to see that inquiries differ considerably in regard to the amount of repetition, or multiplication of instances, which is actually thought desirable by the leading men engaged in the work. And on a comparison of these differences it appears that they depend upon two varying factors mainly—the extent (or vagueness) of the question we happen to have in view, and the amount of knowledge we already possess of the action or process observed. At the

beginning of any inquiry, when both our aim and our knowledge are at their vaguest, the necessity of gathering even chance suggestions is strongly felt. There is then least waste of time caused by loose and amateurish work, such as Darwin used to call—speaking of some of his own—“fools’ experiments.” But by degrees order emerges out of the chaos. Along with a mass of facts which tell us nothing and lead nowhere, a few pregnant suggestions are sure to come. These the dull mind overlooks, while the active mind seizes on them as a detective on a clue, and from that time forward the inquiry begins to lose its random character. The experiments are thenceforth more and more carefully planned, and their results more and more foreseen, until at last we can ask from Nature a definite question and get from her a definite answer. The “prudent question” cannot be asked until we have got some way towards understanding the occurrence, and at first it is unavoidable that

time should be wasted in casting about for clues.

The two reasons for multiplying observations may, therefore, be reduced to one,—insufficient knowledge of the details of causal connection in the facts observed. On such knowledge, already somehow acquired, depends our power both of making inquiries definite and of giving them a satisfactory answer. The fact, thus admitted, that we cannot at present understand the first beginnings of our knowledge of Nature, is of the same relevance as the fact that we cannot at present understand the first beginnings of life, or the creation of matter. We can, at any rate, partly understand the conditions of their change and progress; and that may in the meantime content us. So long as the first beginnings of knowledge remain unexplained, it remains true for us that the secret of progress in Science consists in using the less puzzling details of an occurrence to explain those that are more puzzling.

Scientific inquiry is, in this respect, just like the work of a detective or of any one who tries to see how a conjuring trick is done. The chief difference is that Nature, unlike the criminal or the conjurer, can often be forced to repeat (very nearly) a performance which has been incompletely observed, and so to give us another chance of discarding the accidents and getting the bare essence a little better revealed.

## CHAPTER XIII

### GENERAL RESULTS

OUR object has been to reach a conception of the general nature of "argument" (or battle between belief and doubt) which shall be a little less abstract, less artificially simplified, than that which the traditional logic has provided. It is easy indeed to discover that "logic" is clumsy and stiff-jointed,—many a careless reasoner has got so far,—but the difficulty is to form an adequate conception of the complications involved in the actual process of argument, and especially to express that conception in satisfactory language. For language also is apt to be stiff in the joints.

Now that we have viewed the process of

argument in some detail we can, however, say what feature in it has most obtruded itself upon our notice. Briefly, it is the *composite character of its component parts*. Somewhat as the subdivision of a magnet brings us no nearer to the actual separation of its poles, so the analysis of an inference brings us no nearer to the actual separation of the elements which (ideally) combine to build it up.

We began by noticing that a judgment is only disputable in so far as it is an inference from something undisputed, something taken as fact. But what we shall take as fact in a given case remains an open question until we choose to close it "for the sake of argument"; so that, although every reasoned judgment is composed of fact and inference, its fact element is itself disputable (except by agreement not to challenge it), and contains in itself an inference from a fact. The process of disputing a "fact" being thus the same as that of disputing an inference, it was



needless to consider the former separately. In discussing the objections to an inference we include the only possible objections to a "fact."

But inference itself can be seen to be a highly complex process. We may indeed—at least in the case of the simplest and most familiar inferences—speak of it as depending upon a single "generalisation." Of inference in the abstract this is true enough; and where a concrete inference is easy and familiar we are often able to express its major premiss in a compact form of sentence, like "All men are mortal." Our power of doing this successfully, however, is conditioned partly by the vocabulary at our disposal, and partly by the disputability of the conclusion, or (what is in the end the same thing) by the degree of exactness which happens to serve our purpose in the special inquiry. As the demand for accuracy increases, we are less and less able to bring our conclusion under a simple rule. Simple

rules of conduct, for instance, which are good enough for ordinary, unimportant occasions, are seen to need modification by other conflicting rules when greater interests are at stake. As soon as a conclusion becomes seriously disputed, or as soon as some finer purpose demands a closer approximation to the truth, the need is felt of looking behind the formula in which our generalisation is expressed, and either correcting and amplifying that formula or discarding it in favour of the more tacit kinds of insight, such as those employed by the artist or the skilled investigator.

In proportion, therefore, to the difficulty of the conclusion,—or to the care we choose to take in reaching it,—our major premiss may be seen as a web of separate threads of generalisation, rather than as a single generalisation compact and indivisible. Just as, in disputing a “fact,” we have to separate within it a more from a less disputable part, so in disputing a generalisation: some of its

component parts will be found to be of better quality than others, and the most economical plan is to attack first those parts that are weakest. But to attack any one component part is merely to carry the unravelling process further still,—to distinguish within the doubtful piece of generalisation a more and a less disputable part. No human being can see the real end of such analysis ; we make an end of it, for the time, when our practical needs at the time are satisfied or our patience or powers exhausted.

The process of criticising generalisations may thus be described as that of disputing connections which have hitherto been undisputed, and so finding within them a more and a less disputable part,—a part which approaches the type of “theory” and another part which approaches the type of “fact.” This description applies equally whether we are attacking a formula consecrated by wide acceptance, or a mere glimpse of connection newly obtained by ourselves. In either case,

or in any case that lies between these two extremes, the generalisation so regarded as doubtful is the interpretation put upon an occurrence which is regarded as fact. The description applies equally whether the occurrence has happened only once, or any number of times; the only fault to which all interpretation of fact is liable is that of confusing the essential circumstances of the case with those that are accidental. We cannot be perfectly satisfied in taking X universally as a sign of Y, if their connection depends upon conditions which the names "X" or "Y" leave out of account.

The art of improving our generalisations, therefore, whether those that have long played a part in Science, or those that are still on trial or hardly formed as yet, depends upon the insight we can anyhow obtain into the working of causes in detail—into the *conditions* under which things happen. The aim of Science is to make our generalisations less and less "empirical," by understanding

as far as we can *why* such and such events have occurred together or in succession. Its method consists in analysing the occurrence into a tissue of smaller occurrences, and then distinguishing among them those which it is (comparatively) safe to assume to be cases of direct causal sequence. Professor Marshall Ward's conclusions about the behaviour of *Botrytis*<sup>1</sup> do not yet appear to have been overthrown; but it has been definitely found unsafe to assume, with the two French professors mentioned above,<sup>2</sup> that a lapse of twenty-four hours makes no essential difference to the splenic fever infection.

It remains now briefly to indicate what I conceive to be the practical use of departing from the ordinary abstract view of the process of Inference, and of following out its complications in the direction here suggested. In some respects its justification is much the same as that of philosophy in general. It

<sup>1</sup> See p. 156.

<sup>2</sup> See p. 95.

seems best to admit freely that for many of the ordinary purposes of life these subtleties are not only not wanted, but that their recognition may even be at times a hindrance to the act of promptly "making up our minds." So far as ignorance is power, there seems little use in claiming that knowledge has a practical value. But in making this admission we may be as far as we please from playing into the hands of the actual party who incline to the side of ignorance in general against knowledge, or even of common sense in general against philosophy. When we come to speak not of opposed ideals but of the actual parties who take the opposite sides, we find certain false assumptions made by the parties in question. They are apt to assume, for instance, that no middle ground is tenable; each party sees certain truths, and is apt to suspect that their opponents fail to see them.

In spite of all the excuses that may be found for the habit of making partisan assump-

tions, these assumptions themselves are, in fact, by no means always warranted. Supposing it to be the case, for instance, that there is a class of philosophers who condemn common sense as simply inferior, thereby provoking sensible people to retort that philosophy is unpractical and arrogant, we are under no compulsion to follow those philosophers. There is another class, to which we may quite as easily belong, who would admit all that common sense, when unprovoked, contends for, and who yet see certain uses in philosophy. We need neither fail to recognise the occasional value of ignorance, rough and ready views, artificial clearness of distinction, nor yet the occasional value of knowledge, deliberation, and subtlety. Rather, we may be compelled to think that any *general* balancing of the one set of qualities against the other is futile. We may consider that it is every man's business to decide for himself how much time he can spare, on a given question, for looking behind

the current views and phrases, and trying to dig down into the facts of the case.

While therefore it would be foolish to pretend that every one ought at once to abandon the traditional logic, with its artificial simplifications and short cuts, in favour of some fuller and truer view of the facts of argument, we may yet perhaps claim for the attempt to reach the truer view the same sort of value that attaches to the careful (if "unpractical") work of the specialist in science. It is often difficult to foresee what may be the future results of admitting a piece of recondite truth, and it is always possible to contend that the world has hitherto done very well without it. But in most cases some glimpse, at least, may be gained of the present uses for a piece of truth, even though the need for it does not appear exactly pressing.

Two chief present uses may, I think, be found for the truths put forward in the above account of the nature of inference. As noticed



in Chapter V. and elsewhere, the recognition of a ground of inference as complex, instead of as consisting in a "major premiss," leads to a fairer treatment of any disputed conclusion. The most remediable source of unfair criticism of a judgment lies in taking too literally and sharply the express statement of the grounds. This treatment, in its most formal and pedantic shape, assumes that a reason given for a belief must be a premiss of a syllogism, and must therefore commit the reasoner to a second premiss which is doubtful or absurd. We have seen that the reasoner has only himself to blame if the appeal to "Logic" frightens him into any such open trap. The more we recognise the actual complexity of the grounds of any difficult judgment, the more we shall understand the function of criticism to be not that of tripping up an unready opponent, but of joining in an attempt to guard a conclusion against hidden sources of error; an attempt, therefore, to find what strain a conclusion (already

roughly true) will bear ; to find some of the occasions on which it ceases to be true ; and so to help forward its approximation to the truth.

The avoidance of unfairness or solecism in controversy is not, however, the only present use of recognising the tangled nature of grounds of inference. A still more important result is the cultivation of a proper distrust, first of words and phrases as compared with facts, and secondly of "facts" as compared with truth. The gradual education of the observing and reasoning powers is very much the same whatever be the department of facts observed. As children, we grow up among a set of current notions and formulas adapted rather for hasty than for careful purposes. If the facts in which we are interested be those of business, society, or any similar department of knowledge, whose principles are not expressly taught at school, the line between the more and the less knowing people is never very clearly marked ;

but if the facts be those of any recognised Science or profession, a period<sup>1</sup> of studentship has next to be gone through—a period still of acquiring *accepted* notions and formulas, accepted, however, by those who have advanced beyond the simpler popular views. Finally comes the stage in which we “gain experience,” or, in other words, discover for ourselves the exceptions to the rules, and so carry on the refinement of current notions and formulas further.

If then the view here taken of the process of argument be on the whole truer than that which is taught by the traditional logic, we may claim for it the same sort of value that experience of the facts has in any other department of knowledge. But the peculiarity of logical inquiries among all others is that the field of their application is so wide. Experience in Logic, if we can gain it, should help forward the process of gaining experience in any other direction

<sup>1</sup> These periods, or stages, of course overlap in practice.

where promptness of judgment is not a matter of chief importance. In Science especially there is room for great deliberation and patience. There, if anywhere, we may safely refine our truths to the utmost. Those, therefore, who are students but not yet masters of a Science may find it useful to supplement their present view of the general nature of scientific evidence by forming an opinion on some of the questions here discussed. Their present view is likely to be much influenced by the convenient inexact phrases which are freely used in common talk about evidence, and proof, and mistakes in reasoning. Where no great degree of accuracy is required such phrases serve a useful purpose, but the moment our standard of accuracy is raised above that of the discussion of trifles, the current phrases become more or less a stumbling-block. Thus the ordinary "axioms" about Cause and Effect turn out, when strictly taken, to be either untrue or

else inapplicable. Any such axiom either admits of exceptions or else its application to any particular case can only be satisfactorily made after the inquiry is finished. For instance, though in the abstract a Cause must precede its Effect, an actual Cause and Effect (or what seem such to our best observing powers) may be so entangled that we cannot separate them ; and though it be true that the same antecedent is invariably followed by the same consequent, the practical difficulty is that of knowing, *before* the consequent occurs, how far the antecedent is the same.

Besides rough and ready axioms, and cautious truisms, expressions like "essence" and "essential" have crept into common talk. As there used, they are used without a hint as to the difficulty of applying them. In connection with resemblance and difference, the adjective "essential" is often taken as almost synonymous with "considerable," and almost entirely without reference to

special occasions and special purposes. There is no popular error from which the student of Science must more resolutely shake himself free than the notion that resemblance and difference are to be weighed and measured in the lump, as an amount;<sup>1</sup> their importance depends not on their being considerable, or striking, but on their relevance to some particular question. The same resemblance or difference is essential for one purpose, accidental for another, quite independently of its general extent. Thus the resemblance between a good half-sovereign and a bad one is greater than that between a good half-sovereign and a good half-crown, yet the resemblance between the latter pair is (on the question of value) more

<sup>1</sup> This popular confusion as to the use of the word "essence" or "essential" would be less important than it is, if it had not also to some extent invaded Logic. Whenever "degree" or "amount" of resemblance or difference is spoken of, the student must remember that, for all purposes of reasoning, a resemblance or difference is great or small, not according either to its power of striking the observer's notice, or to the *number* of "points" (or details) into which it may be analysed; but according to the importance of its details in regard to the matter in hand.

“essential” than that between the former pair.

An equally effective hindrance to those who are beginning to put together their notions of Scientific Evidence is caused by the rigid way in which popular thought—appropriately for its own hastier purposes—conceives of certain distinctions. A superstitious reverence for words, and for their underlying distinctions, is, in every department of knowledge, one of the most unfailing signs of a superficial view of the facts. Every distinction that language makes is, by comparison with the facts it refers to, abstract, artificially simple, and made for convenience merely. All opposites, like all gaps or distances in Space or Time, are connected by an intermediate region. It is, however, specially to one set of distinctions that our attention has here been directed, and for our present purpose the wider application of the doctrine may be left out of account. We have recognised its truth in

regard to the distinction between assertions of fact and assertions of theory ; between generalisation, analogy, and circumstantial evidence ; between “merely empirical” and “scientific” generalisations ; between observation, generalisation, and the criticism of generalisations ; between observation of things and of occurrences ; between causal sequence and sequence other than causal ; between cause and effect ; and between the quantity and the quality of evidence. All these distinctions, we have found, possess a value which depends upon their not being pressed too far.

The extended meaning given to the term “argument” in this book has for its justification not only the fact that the process of reasoning to convince ourselves is the same as that of reasoning to convince other people, but also the hope that the former kind of argument will gradually supplant the latter. Argument, in the sense of controversy, seems to be on the whole less



seriously taken than it used to be; argument, in the sense of care in forming opinions, seems to be on the whole more seriously cultivated. There seems to be a growing recognition of the fact not only that, as some one has said, "to refute a man is the worst use you can put him to," but that it is the most unlikely way to convince him. We all prefer to have our erroneous reasonings prevented rather than cured; we find that by taking care to criticise our own judgments as we form them the hostile force of outside criticism can be greatly weakened. At any rate, the opportunities for discussion and controversy are nowadays not very numerous, outside the law courts and parliament,—and there the search is almost openly for plausibility and not for truth. The tricks of sophistry have, however, here been left out of sight, not under the assumption that we never use them against ourselves, but rather because the subject is so much more intricate than the one we have

been discussing. I think we shall be in a better position for dealing with it when we have reached a satisfactory view of the nature of argument on the simpler assumption that it is throughout inspired by perfect candour. This side of the subject seemed therefore to deserve a prior treatment by itself. Though the simplification is of course artificial so far as our reasonings are really influenced by self-sophistication or our discussions by party interest or personal conceit, our actual reasonings are not entirely subject to these influences. It is often possible to know, in given cases, that what an enemy would call self-sophistication a friend would more soberly regard as genuine doubt ; and we have all experienced discussions where personalities and partisanship were on the whole subordinated to a search for truth. A knowledge of the process of unemotional argument can, therefore, be put to at least occasional service.

# APPENDIX

## A

### NOTES ON THE TECHNICALITIES OF LOGIC

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with a few alterations.*]

ALMOST every one would admit that the technical terms of what is usually taught as Logic are to a great extent survivals from philosophies now very largely superseded. As exercises for the student's memory, and as affording material for examination questions, they may still have a value. To the thorough-going student of the history of philosophy they will probably always be interesting. But for any other purpose, except that of causing confusion and hindering progress in a subject which is difficult enough even without them, they have long been losing the value they formerly had. "Logic" bristles with terms which have gradually sunk out of use, as argument has ceased to be a game with rules laid down by authority.

An attempt to make a complete list of these high and dry technicalities is here unnecessary, since in their case the best reform one can propose is to follow the practice

of common-sense and drop them quietly out of remembrance.<sup>1</sup> In order to do this we need not know beforehand precisely which they are. We need only adopt the simple rule that the first question to be asked regarding any logical technicality is, What is its actual value in helping us to understand the process of argument? At any rate, the terms proposed as worth preserving must show other credentials than the fact of having been handed down to us, before we can safely assume that there is any such value in them, for us whose philosophy is so different from that of the Middle Ages.

Still an instance or two may be useful as showing the kind of technicality for which it is hard to find any practical justification. Those who have ever had to teach elementary logic for examination will remember the stimulating effect produced on a class of beginners when the meaning of some sonorous and respectable word is explained to them,—some word like *syncategorematic*, *epicheirema*, *polysyllogism*, or the *Goclenian Sorites*. Such words are welcomed with (comparative) eagerness as something definite, something that can be learnt, and reproduced at the proper time on paper. The Goclenian Sorites seems to be an especial favourite, probably from the simplicity of the contrast between it and the Sorites which the books call ordinary. Of further examples the first that come to hand are: *relative* and *privative* terms; *exponible*, *copulative*, *remotive*, *exceptive*, *exclusive*, *indefinite*, *plurative*, *limitative*, *propositions*; *sub-contrary* and *subaltern* opposition; and most of the

<sup>1</sup> Except, of course, for those advanced students whose interest lies chiefly in the history of the subject.

machinery of the Syllogism, with its *Barbara*, *Celarent*, and the rest of the "valid moods."

But such technicalities are mentioned here only to be dismissed entirely from consideration. There remain also in Logic a good many technical terms of a different sort, possessing a value which is not merely historical; and it is these that appear to deserve reformation instead of burial. The suggestion I would make in regard to them is simple enough in idea, and perhaps we need not despair of it being made capable of application.

In the last two or three centuries a great change has begun to come over our philosophy, including that freer and less exact philosophy which is known as Common-Sense. The change is still in progress and is far from being accomplished, but its general tendency is plain to see. It consists especially in our gradual escape from a subtle form of mental slavery,—from the bondage of words. We are learning that words, after all, are only counters—instruments of expression—and that every distinction drawn by language is open to criticism in the light of our knowledge of facts. We are becoming accustomed to find that a distinction may be perfectly sharp in *idea*, while the actual classes distinguished shade off into one another and so do not fit either of the sharply-contrasted names.

The technical terms of Logic, like all other terms, imply distinctions drawn. If we name, for instance, kinds of term, or kinds of proposition, or kinds of argument, the process is plainly one of distinction. And the same where we divide arguments or propositions into their component parts, or separate the "meaning"

of terms into connotation and denotation. On some distinction or other, every descriptive term is based. And the reform here proposed is merely that of recognising the real (or actual) roughness of all the distinctions drawn in Logic, in spite of the sharpness with which they may appear to be drawn. Such recognition will have various effects, and the best way of understanding what is involved in it will be to trace out some of those effects in detail.

First let us admit, regretfully or otherwise, the fact that any proposal for a wholesale alteration of logical terminology is unlikely to meet with general acceptance. The most one can reasonably hope to do is to drive the thin edge of a wedge a little further in. Instead, therefore, of suggesting a set of new technical terms, or even new ways of defining the old ones, I here only try to express certain reflections that may accompany our use of the old technicalities, in much the same way as our remembrance of the fact that the earth revolves may accompany our use of the word *sunset*. Let us keep the old technicalities, by all means, so long as we can anyhow render them harmless. This plan is rather more troublesome, perhaps, but will cause less offence to our conservative instincts.

To begin with the most central technicality of Logic : what is a *proposition* ? This term is commonly made to do duty for two very different meanings. It is used indifferently for the assertion expressed in a sentence, and for the sentence in which the assertion is expressed.<sup>1</sup> The simplest remedy would consist in avoiding the word

<sup>1</sup> If the reader, by chance, finds it difficult to separate sentence

proposition altogether, and substituting for it one of the words *assertion* or *sentence*, whichever we happen to mean ; but we may also attain the same end by keeping the word proposition in use, and merely remembering its defects on the proper occasions.

It is not difficult to see how the confusion between assertion and sentence arises. The *ideal* proposition is an assertion, but the *actual* proposition is always a sentence, just as the ideal nobleman is noble, while the actual nobleman is a titled man. In the case of assertion and sentence, however, there is more excuse for the failure to distinguish, since we cannot conceive what any assertion is or means except by putting it into a sentence. And though a sentence without a meaning may easily be invented, this is practically never done.<sup>1</sup> Sentences, as we meet with them, are used for the purpose of conveying meanings, however imperfectly they may succeed in doing so. Hence, as soon as we distinguish kinds of assertion, and ask what actual assertions belong to each kind, we very naturally bring forward not assertions but sentences to illustrate our distinctions. Thus we give the *sentence* "All men are mortal," as an instance of the universal from assertion, even in thought, that is the very thing I complain of, as one of the ill effects of Logic as commonly taught. Two suggestions may here be of special service to such a reader ;—that the assertion is not necessarily something *revealed* by the sentence, but something *revealed or concealed* by it ; and that the distinction between assertion and sentence is analogous to that between *nobility* and *rank*. No doubt there are people who cannot sever these latter things, even in thought. Yet the ideas are distinguishable, as soon as we learn that rank (external form) may either reveal nobility or conceal the absence of it.

<sup>1</sup> The exceptions to this rule are induced to complete unimportance if we reflect that a (so-called) "meaningless" sentence produces no fallacy until a *wrong meaning* is put upon it.

affirmative *assertion* ; and by calling both the example and that which it exemplifies a *proposition* we hide from ourselves whatever risk there may be in the above proceeding.

The inevitable result is that "Logic" gives us a classification of sentences in place of a classification of assertions. Partly through the labours of Aristotle and the Schoolmen, partly with the aid of more recent grammar, we are in possession of a fair amount of knowledge of the sentence-forms that meaning commonly takes. It is doubtless true, for instance, that when we say : "All S are P," we commonly mean to express the "universal affirmative" meaning ; these common forms were not invented by philosophers for amusement, but to a great extent arise from the general consent of practical men who desire to find the best means of expressing their thoughts. All this may be admitted to the fullest extent,—in fact, every writer knows that he must on the whole obey grammar and custom if he wishes his readers to understand him—and yet the opposite side of the truth should also not be forgotten.

For, no grammar—no reflections on custom—can adequately represent so complex and shifting a set of phenomena as those of the expression of meanings. We may do full justice to "general consent" and yet admit that language-forms are largely an accident of time and place, not to speak of those finer differences that depend upon the varying mental constitution of different people, or upon the degree of assertiveness with which the assertion happens to be made. The failure of "Logic" to cope with such facts as these—facts not exceptional or unimportant, but of immense and direct practical weight in



dealing with any assertion or argument—is so notorious, that nowadays to appeal to Logic in support of any opinion is almost enough to spoil our chance of persuading common-sense to accept it. “I will not admit that the whole is greater than the part, unless you tell me how you are going to use the admission.” It is not, in the end, the words that make a meaning, but the uses to which we put them.

A sentence may thus not only carry different meanings to different people, but may represent to the same person different kinds of assertion indifferently. This will perhaps be disputed at first by those whose minds are full of the grammar-logic here attacked ; but, if they wish to avoid begging the question, they will rather notice that the answer yes or no depends on the view we take as to the nature of *meaning*, while this again depends on whether we do or do not keep clear the distinction between assertion and sentence. So long as we think of a meaning as itself a sentence (or as something necessarily revealed by the sentence) instead of as the assertion revealed or concealed by the sentence, we are hardly ready to recognise its shifting character,—we tend to suppose the “meaning” of a sentence to be something inherent in the sentence itself (like specific gravity in this or that kind of substance), not something dependent on the intention of the parties using it (like the force of a mathematical symbol). We thus become grammarians rather than logicians, and spend our energies on searching for the “logical meaning” of the words, *some*, and *or*, and similar expressions which in practice have more than a single meaning.

Let us follow out a little further the results of remembering that assertion and sentence are not the same thing. Suppose, for instance, we try to make a classification of assertions,—if only for tacit use, along with the classification of sentences that the present Logic so carefully provides. As soon as we recognise that it is not the words that make a meaning, but the use to which they are put, two important consequences follow: words, we must then recognise, get their meaning and character from the assertions they help to express, and assertions get their meaning and character from the arguments into which they enter; or, more exactly, from the purpose they are made to serve in some argument,—not necessarily the purpose for which “most people” use them on most occasions, but that for which the assertor uses them at some particular time.

Kinds of assertion thus become, in effect, kinds of *use in argument*. And here, under whatever names<sup>1</sup> we choose for the kinds distinguished, the most important division is that between the assertion whose function is to state the general ground of the argument, and the assertion whose function is to state the particular application of that general truth. Each of these without its counterpart is ineffectual in argument and so devoid of “meaning,” and so devoid of existence as an *assertion*; the major without a minor is “empty,” the minor without a major is “blind.” The former corresponds to the theory which helps to give a fact its meaning, the latter to the fact which helps to give a theory substance. Facts and theories (however

<sup>1</sup> *E.g. major and minor premiss; or inferential (or conditional, or general) and categorical (or predicative) assertion.*

inseparable from each other) are the whole material out of which arguments are woven. In order to justify a conclusion we must appeal to facts (*i.e.* to supposed facts), but since, even where the facts are admitted, such an appeal may be irrelevant, something more than the bare fact is needed, namely, assurance of its relevance for the purpose. But what do we mean when we claim that a fact is relevant to the proof of a conclusion? We always mean that in other cases, *analogous* to the one before us, a similar fact has been known to justify a similar conclusion. We mean that the fact appealed to does not stand alone, is not entirely *sui generis*, but belongs to a class of facts whose causes and effects are known,—known sufficiently for the purpose of our assertion. In this way we refer to causal theory whenever we use a “fact” for proof; and without such reference the fact would have no argumentative value. Similarly, in the absence of fact perceived, the general knowledge which enables us to use that fact for inference is barren. Fact and theory, taken together, are effective; either by itself is null. A rule or principle that can never be applied in particular cases, and particular cases that are not cases of a general rule, are each equally incomplete,—at best are waiting for a future (a potential, not actual) meaning, purpose, and value. The only use of any fact is to be connected with some generalisation; the only use of any generalisation is to be connected with particular facts.

If we were to define the two kinds, *inferential* and *categorical* assertion, as above, any “proposition” may exemplify either kind, by being put to either use; but only during such use. What are commonly called “singular

propositions," for instance, may be made to serve the purpose of major premisses with perfect ease. A sentence, that is to say, with a singular name (even a proper name) as its "Subject," may be used to make an assertion which is in its purpose purely inferential. Take the case, for instance, where the fact of reaching a certain station convinces us that we are near the end of a journey. The sentence "Kentish Town is near the end of our journey," would, I suppose, be commonly classed as singular; and yet, in connection with the minor premiss "Here is Kentish Town" the *assertion* becomes inferential. It is a grammatical accident that we use the categorical form of sentence, instead of saying "*If* this is Kentish Town, then," etc.

One result of our view is that a sentence, taken by itself, never declares its logical character except in a rough and provisional way. An assertion only declares its character because no such thing as an independent assertion exists, any more than an independent *term* (as contrasted with *word*). As soon as there is meaning at all, there is the polarisation of thought into major and minor premisses. Any given assertion—any understood sentence if it asserts, and is not merely truistic—may be regarded, at our choice, either as itself a conclusion, that is to say as the combination of a major and minor premiss, or as forming one of the premisses out of which a new conclusion follows. When regarded as itself a conclusion, its logical character is not yet declared; it may be put to either use in the future. It is only when and while it is itself a premiss that it has any logical character, in this sense, at all. It is thus only in "promise and potency"

that unattached inferential or categorical assertions can be said to exist. Their actuality begins with their mutual dependence.

Let us next ask what becomes of the division into affirmative and negative propositions. Every assertion, we must remember, may be regarded as giving either the answer "yes" or the answer "no" to a corresponding question; and it must also be remembered that any question admitting of such an answer is one of a pair of questions ("contradictories") such that if one be answered "yes" the other must be answered "no," and *vice versa*. Hence no assertion is more affirmative or more negative than any other. This need not, of course, prevent our recognising to the full the practical difference between affirmation and denial in certain cases,—a difference in definiteness of assertion. But we cannot make exactly the use that is commonly made of the distinction.

As regards *universal* and *particular* propositions, this distinction becomes absorbed in that between the acceptance and the rejection of a proposed inferential. Apart from a system of sentence-forms, we do not want to know whether a proposition is "universal" or "particular" for any other purpose than that of knowing whether it has the energy to serve as a ground of inference when it meets with a relevant fact, or whether (being a mere denial of an opposite ground of inference) it remains neutral until it can be made more definite and assertive.

*Terms* are arrived at by analysis of assertions; we cannot think of terms as being taken separately and coupled together to form an assertion, though of course words are habitually thus coupled together to form a

sentence. But the term is an abstraction from the assertion, and takes its whole character from the purpose it happens to serve in asserting. Major and minor premisses (or inferentials and predications) divide into terms differently: the former into *antecedent* and *consequent* (or *sign* and *signification*), related to each other so that, given A, C is asserted to follow,—or, as it may sometimes be conveniently regarded, A is asserted to *indicate* C; and the latter into *Subject* and *Predicate*, related to each other so that S is asserted to belong to the class P,—or, as it may otherwise be expressed, to have the attributes essential to that class.

Every term is thus either S, or A, or C, or P, in some assertion; and in each of the three latter usages it is *general*, or *predicative*, in character; while, when used as S, it is either *proper* or *quasi-proper*,—does not require definition, in order to serve its purpose. “Reference-name” would be a convenient technicality for a word when used as the S term of a minor premiss. It does not matter whether such word denotes an individual, a class taken collectively, or a class taken individually. During the time that an assertion is a minor premiss, its S does not require definition in order to serve its purpose.

Next, let us try to see what occurs when a mind which is full of the notions above sketched out looks through an ordinary text-book of elementary logic. The chief result is that short work is made of most of the puzzles that are wont to confuse the student and to lead him at times to shake the teacher’s confidence with troublesome questions. I will select only a few of the best-known of these, in illustration.

(1) *Are abstract names general?* In order to be "general," a name must be descriptive,—else it has no connotation and therefore cannot be "correctly affirmed, in the same sense, of each of an indefinite number of things, real or imaginary." But a name becomes descriptive only by being used to describe—*i.e.* used as P in a predication, or as A or C in an inferential. And, passing over the difficulty<sup>1</sup> (just here irrelevant) of distinguishing in practice between abstract and concrete names, it is plain that in whatever way "abstract" names be defined they may be used for any one of these purposes, and also as S in a minor premiss. The assertion expressed in "*Familiarity breeds contempt*," for instance, may be either major or minor premiss. Hence this question becomes a real question no longer; does not admit of a choice between "yes" and "no." Abstract names, like all other names, *may* be general,—are general when they happen to be so, in fact.

(2) *Are proper names connotative?* If connotative means descriptive, then all "proper" names, when used as P in a minor premiss—*e.g.* in the instance given above, "Here is Kentish Town"—become connotative.<sup>2</sup> If, on the other hand, a proper name be *defined* as

<sup>1</sup> The distinction between abstract and concrete names, when defined as that between the names of *attributes* and the names of *things*, is only a rough distinction until we are in a position to define "thinghood" perfectly. By a perfect definition is here meant what I have elsewhere (*Distinction and the Criticism of Beliefs*) called an "applicable" one; a definition such that by means of it we can decide on which side of the line any actual specimens presented to us should be placed.

<sup>2</sup> The connotation (so far as the purpose of the moment is concerned) being given by the major premiss.

"given merely to distinguish an individual person or thing from others," and its application as being "independent of any special attributes that the individual may possess," then "proper" names are defined as non-connotative; *i.e.* we must find out first whether or no a given name is connotative, before we can say whether it is a "proper" name.

(3) *Verbal and Real Propositions.* When we remember that all distinctions in Logic are abstract, we shall regard much that is commonly written about verbal and real propositions as illusory and confusing. The use of this particular distinction is mainly in connection with the question as to the material truth of any given conclusion. Is either premiss "merely verbal"? Then the conclusion is so too. Are both premisses "real"? Then so is the conclusion.

A perfect (or applicable) definition of real and verbal propositions cannot be found; in practice, verbal propositions cannot be distinguished from real ones, by mere inspection, or by reference to the "usual" definition of the terms, except in a rough and provisional way. If we take some sentence like, "Homer wrote the Iliad," or, "7 and 5 are 12," apart from all context, and affirm positively that it is "verbal," or on the other hand "synthetic," we forget that, whatever may be true of sentences, assertions get their character from their use. It is for the assertor to explain, when called upon, whether his assertion is intended as a mere postulate or not; in the absence of such explanation, it may be either postulate or doctrine,—though of course there is often a strong presumption in favour of one or the



other. For instance, I find it difficult to imagine a case where the sentence, "a triangle is a three-sided figure," could be used to express a "real" assertion, or where the sentence, "Brutus killed Cæsar," could be "verbal." But all that we are here concerned to notice is that no rule can be given for distinguishing, in doubtful cases, verbal from real assertions, unless or until we can get a declaration from the assertor himself. The same form of words—say, "a straight line is the shortest distance between two points"—may be used either as a postulate of meaning or as a statement of fact.

(4) Next let us look at the doctrine of *Conversion*. Faulty Conversion, as every one admits, implies a mistake as to the meaning of a sentence. The fallacious reasoner assumes, for instance, that the sentence, "All S are P," means that "All P are S." It is plain, therefore, that rules of Conversion are rules for the correct interpretation of sentences; the "legitimate converse" of an *assertion* is only the same assertion (where meaning and assertion are one) though differently expressed. But instead of stopping at this negative result it may be worth while to go a little further, and notice that when the machinery of *Barbara*, *Celarent*, etc., is discarded, the practical need for rules of conversion even of sentences almost disappears. All we require to remember is that in interpreting sentences it is generally unsafe to assume that the terms can simply change places in regard to the relation between them. A good many relations, of course, like *equality*, *cousinship*, *nearness*, etc., admit of simple conversion, but precisely those two relations—indication and predication—which rise into

chief importance as soon as we distinguish the premisses on which a given conclusion rests, do not admit of it. If the terms are simply transposed, the relations must be twisted round ("indicates" into "is indicated by," or "is" into "includes"); if the relation remains unaltered, both terms must be changed into their contradictories.<sup>1</sup> It should further be noticed that, under this view of the matter, the same rule serves whether the proposition answers "yes" or "no" to its question. For instance, "X indicates Z" converts into "non-Z indicates non-X," and "X does not indicate Z" converts into "non-Z does not indicate non-X." (This latter pair are often more conveniently expressed as "Some X are not Z," and "Some non-Z are X.")

(5) *Predication and Existence.* This group of difficulties also becomes less important or puzzling when we keep assertion in view, rather than sentence. The question is sometimes raised whether, if we say that "All S are P," we imply that any S exist. Our answer would be that (when any definite meaning is given to "existence") though a given assertor might intend to imply it, he certainly need not do so. In the case of major premisses the conditional (or hypothetical) character of the assertion lies on the surface for all to see. Whatever implication of existence the assertor may intend is beside the purpose of that particular use

<sup>1</sup> There are perhaps many cases where Grammar would say that a term had no contradictory. In the case of "proper names" it must be very seldom that Logic would not be content to abide by Grammar in this respect; but whenever we feel how convenient a negative name would be, which is not at present in use, we begin to rebel against Grammar.

of the assertion. In fact, Grammar often chooses the hypothetical form of sentence for a major premiss. But in the case of minor premisses the existence of S is so commonly implied that the question whether it is *necessarily* implied becomes much more plausible. However, the process of reducing opponents' assertions to absurdity plays a considerable part in argument, and for that purpose the real existence of S is often not implied, even in minor premisses. Wherever, in fact, our conclusion is that S does not exist (*e.g.* "miracles do not happen"), our minor premiss cannot imply an opinion of *our own* that S exists. There may also, perhaps, be other cases where the minor premiss is a "verbal proposition." No doubt Mill was right in saying that "real" propositions—so far as they predicate—do imply the real existence of S. But he seems not to have been fully aware of the difficulty of saying off-hand which propositions are real and which are verbal.

Again, the fact of an assertion being "particular" does not seem to prevent its being verbal and so non-existential. We can therefore only agree with Dr. Venn's views to a limited extent. Particular assertions, being mere denials<sup>1</sup> of indication, can never be anything but minor premisses,<sup>2</sup> and are therefore on the whole more likely to imply existence; but there seems no reason *à priori* why the "Some S" that are spoken of should not be conceived as merely "Some so-called S,"

<sup>1</sup> See above, p. 209.

<sup>2</sup> *I.e.* in the sense in which "minor premiss" is defined on p. 206; according to which, the moods *Disamis*, *Dimaris*, and *Bokardo* (and three others) have their premisses arranged in inverted order.

with the tacit mental reservation that no rightly-called S exist. Dr. Keynes (*Formal Logic*, part ii. chap. viii. § 106) says that unless particular propositions are made to imply the existence of S, the doctrine that A and O, or E and I are contradictories no longer holds good. This view seems to be justified where the form "No S are P" is interpreted as meaning that the combination SP *does not exist*; for then, of course, the form "Some S are P" will only contradict this when it asserts that SP *does* exist. But where "No S are P" means that S and P *cannot* be combined (e.g. "no straight lines are curved"), the particular need only assert that their combination is *possible*. If this latter meaning be held to introduce modality into the propositions, and so to remove them from the class of pure categoricals, there is yet another way in which we may make A and O, or E and I, contradictories without assuming "existence" for S in the particular proposition *any more than in the universal*. In both alike, so far as they are predications at all, *some sort of* "existence" must be assumed for S, though it may be only the wholly indefinite kind of existence that belongs to any nameable thing as such (e.g. a ghost, or a sea serpent), and may be expressly contrasted with—e.g. "real" or "actual" existence. Whenever, therefore, we assert that S *does not* exist, the word "exist" must be taken in some narrower, more definite sense than this which is universally undeniable; and similarly, when we raise the doubt whether the "existence" of S is involved in one form of predication and not in another.

Now, if we take any S whose "existence" (in some *definite* sense) we do not believe in, we find that the A and

O (or E and I) forms of proposition are contradictories nevertheless. *E.g.* either of the expressions "All sea-serpents are half a mile long," and "Some sea-serpents are not half a mile long," might be used where the speaker was only referring to "Your so-called sea-serpents," or to "The sea-serpents we read about but very seldom see," or to "The sea-serpents which exist only in the disordered imagination of a drunken sailor," and so on.

---

## B

### STRUCTURE OF THE SYLLOGISM

[See Chapter VI]

THERE are two different ways in which the structure of a syllogism may be conceived: First (the traditional one), as consisting of *sentences*; and, secondly, as consisting of *assertions*. Sentences which, though different in outward form, are equivalent in meaning,—as, for instance, "No X is Y" differs from "No Y is X," and yet is equivalent to it,—may be regarded as different ways of expressing the *same assertion*. The traditional logic itself adopts this principle to a limited extent. That is to say, it reduces the number of sentence-forms to four, decreeing that if we happen to meet with a sentence which does not exactly fit one of these four forms we must re-arrange the words until it does so. But the same principle may clearly be carried a little further. If meaning, as contrasted with mere outward form, be

what we are seeking, then equivalent sentences should *always* be regarded as the same assertion.

It is plain that this extension of treatment would simplify syllogistic doctrine very much, by reducing the number of forms of Syllogism that we have to remember. It will do this, moreover, at no real cost, since the reduction can be made by means of the very same processes—"conversion" and "obversion"—which the student of the traditional logic is already required to perform. For readers to whom these processes are unfamiliar, the following account of them may be useful.

The four forms of sentence recognised in the textbooks are commonly referred to by the letters A, E, I, and O. As examples we may take :

(A, or "Universal Affirmative") *All lawful acts are defensible.*

(E, or "Universal Negative") *No poachers are sportsmen.*

(I, or "Particular Affirmative") *Some poachers are sportsmen.*

(O, or "Particular Negative") *Some lawful acts are not defensible.*

*Conversion* (and *contraposition*) is, in one aspect, a method of changing the order of the terms in a sentence without introducing any fresh assertion ; in another aspect it is a method of discovering equivalence of meaning in spite of difference in the order of the terms in two given sentences ; but the sentences, before "conversion" is possible, must be already "reduced to logical form." For instance, the change from "Great is Diana" to "Diana is great" would not be called a case of conversion, but of reduction to logical form. On the other hand, the change from "Some politicians are honest men" to "Some honest men are politicians" would be

called conversion. One of the many difficulties arising out of the traditional method, with its half-hearted attempt to get free from accidents of expression, is that of making quite clear the distinction between conversion and "reduction to logical form."

*Obversion* is a method of over-riding the distinction between "affirmative" (A and I) and "negative" (E and O) sentences, by means of an alteration in their predicate term. The distinction between affirmative and negative propositions plainly refers to sentences, not to assertions. That is to say, it breaks down when applied to assertions, since every assertion is both affirmative and negative at once, and can be regarded as either indifferently. Thus "All negroes are human" is equivalent to "No negroes are non-human"; and "Some negroes are human" to "Some negroes are not non-human." The principle of obversion is thus the same as the familiar grammatical truth that two negatives make an affirmative.

The essential parts of a "syllogism" are three sentences (or assertions), of which one is a "conclusion" from the other two,—these others being called the "premisses." The relation between premisses and conclusion is such that the latter is contained (implied or involved) in the meaning of the former when these are taken both together. But it is also essential to a syllogism that one of its premisses shall be a generalisation (or an "inferential")<sup>1</sup>—the assertion of a general rule—

<sup>1</sup> This includes a "singular proposition" when used for major premiss. See the example on p. 208, "Kentish Town is near the end," etc.

while the other premiss must connect the conclusion with it, as a special case coming under that general rule. Every valid syllogism may thus be regarded as bringing the conclusion under a generalisation, though this is sometimes<sup>1</sup> not the most natural way of regarding it.

In the traditional logic, four "figures" of the Syllogism are distinguished, and in each figure various possible "moods," only a small number of which are "valid." The figures are formed by varying the order of the terms in each of the two premisses. The first figure (called by Aristotle the *perfect* figure) corresponds easily with the account just given above,—plainly shows the conclusion as a special case of a general rule; and, taking this figure as starting-point, fig. 2 is formed by converting the "major" premiss only (the premiss which contains, as one of its terms, the predicate-term of the conclusion); fig. 3 by converting the "minor" premiss only; and fig. 4 by converting both premisses at once.

The distinctions of "mood" depend on the nature (A or E or I or O) of the three sentences composing the syllogism. It is enough for us here to notice that, since there are four recognised forms of sentence, and three sentences, and four figures, the total number of possible "moods"—valid and invalid together—is 256. Of these all but twenty-four are rejected, as invalid, with the help of a set of rules; and of the twenty-four valid moods, five are commonly reckoned as of no account, since their conclusions are "particular" when they might be "universal."

Now the same reasoning which leads the traditional logic to regard these five "weakened moods" as hardly

<sup>1</sup> *I.e.* in the second and third figures. See below, p. 229.



deserving a position along with the others, would, if extended and made consistent, reduce the remaining nineteen valid moods to two which include them all; and these two may, in another manner, be reduced to one fundamental form. The merging process was indeed begun, in an incomplete way, by the old "reduction to the first figure."

Why were the weakened moods regarded as unimportant? Because it was regarded as obvious that if two premisses, in any figure, would support an A or an E conclusion, they would also support an I or an O conclusion respectively. But is it less obvious that if any two premisses will support a given conclusion, they will also support the *converse equivalent*<sup>1</sup> of that conclusion? And is it less obvious that if any two premisses will support a given conclusion, then the *converse equivalents* of those two premisses are just as effective? As soon as we begin to regard meanings as "obvious," we have taken a step towards getting free from mere accidents of expression, a step towards regarding syllogisms as formed of assertions rather than of sentences; and having taken that step, we may as well go all the way.

Starting first with the assumption that the ordinary logic is justified in its reduction of all valid moods to

<sup>1</sup> The converse equivalent of an E or an I proposition is its "simple converse"—i.e. Subject and Predicate simply change places; thus "no X are Y," and "no Y are X," or "some X are Y," and "some Y are X," are pairs of equivalents.

The converse equivalent of an A or an O proposition is its "contrapositive" (or simply-converted obverse); thus "all X are Y," and "no non-Y are X," or "some X are not Y," and "some non-Y are X" are pairs of equivalents.

the four contained in the first figure, let us seek, by applying the same method of counting equivalent sentences as the same assertion, to reduce them still further.

These four moods are :

( <i>Barbara</i> )	( <i>Celarent</i> )	( <i>Darii</i> )	( <i>Ferio</i> )
All M are Z	No M are Z	All M are Z	No M are Z
All S are M	All S are M	Some S are M	Some S are M
<hr/>	<hr/>	<hr/>	<hr/>
All S are Z	No S are Z	Some S are Z	Some S are not Z

Now though these four are different from each other when we regard their component propositions as sentences, yet when we regard them as assertions the argument in *Barbara* is exactly the same as that in *Celarent*. For the major premiss "all M are Z" is equivalent to the major premiss "no M are non-Z," and the conclusion "all S are Z" to the conclusion "no S are non-Z"; and if we let the term P stand for Z in *Barbara*, and for non-Z in *Celarent*, the argument in each becomes :

$$\begin{array}{l} \text{All M are P} \\ \text{All S are M} \\ \hline \text{All S are P} \end{array}$$

Similarly in the case of the other two moods ; they merge in the form :

$$\begin{array}{l} \text{All M are P} \\ \text{Some S are M} \\ \hline \text{Some S are P} \end{array}$$

Need we stop here? It is true that we have now got as far as conversion and obversion will carry us ; but whether we stop here, or take the final step of reducing all syllogisms to a single form, depends entirely on what

our purpose in the inquiry is. If our object is to understand exactly the conditions on which the validity of a syllogism depends, there is no reason why we should be content to keep these two forms distinct. They differ, certainly, in minor premiss and conclusion ; and the difference is a difference of assertion, not only of sentence. But they resemble each other not only in the major premiss, but in the fact that in both cases the character, technically called its "quantity," of the conclusion follows that of the minor premiss. In other words, if we take the term S to stand indifferently for "all S" and "some S" (provided always that it stands for the same in premiss and conclusion) then in both forms the argument is :

$$\begin{array}{c} \text{All M are P} \\ \text{S are M} \\ \hline \text{S are P} \end{array}$$

For the purpose of understanding the general nature of the syllogistic process, and the conditions of validity for any syllogism, there is no need to carry in mind any other<sup>1</sup> "valid mood" than this. If any suggested syllogism, when so treated, conforms to the type just given, it is valid ; and if it fails to conform, there is a fallacy in it.

The reduction from nineteen moods to four rests partly on the principle, already noticed, that any premisses which

<sup>1</sup> As a matter of mere verbal expression, I prefer as major premiss the form "M indicates P," or "if M, then P," as being more explanatory, since it is in the *inferential* character of the assertion, rather than in its "universality" that its power of performing its function resides. Similarly, I prefer as minor premiss the form "Here is a case of M (interpreted so widely as to include the assertion "M is true").

will support a given conclusion will support a weaker form of that conclusion ; partly on the principle, already noticed, that if any two premisses will support a given conclusion they will also support the converse equivalent of that conclusion, and that the converse equivalents of the two premisses are just as effective ; and partly on the obvious truth that the mere order of the premisses does not affect the conclusion which can be drawn from them. Thus :—

In *Bramantip*, the conclusion is the converse equivalent of a weakened form of that in *Barbara*.

In *Camenes* and *Cesare* respectively, the conclusions are equivalent to those in *Celarent* and *Camestres*.

In *Camestres* the conclusion is the same as in *Camenes*, while the premisses are the same as in *Cesare*.

Similarly the premisses and conclusion of *Dimaris*, *Disamis*, and *Datisi* are equivalent to those in *Darii*; and *Festino*, *Ferison*, and *Fresison* equivalent to those in *Ferio*.

Then the premisses in *Darapti* and *Fesapo* respectively are stronger than those in *Datisi* and *Fresison*, and yet yield the same respective conclusions, while *Felapton* is similarly related to *Ferison*.

Finally, *Bokardo*, with its O propositions obverted, exactly copies the form of *Disamis*.

Some Y are non-Z	=	Some M are P
All Y are X	=	All M are S
<hr/>		<hr/>
Some Y are non-Z	=	Some S are P

And *Baroko*, with both premisses obverted, exactly copies the form of *Festino*

$$\begin{array}{rclcl}
 \text{All Z are Y} & = & \text{No Z are non-Y} & = & \text{No P are M} \\
 \text{Some X are not Y} & = & \text{Some X are non-Y} & = & \text{Some S are M} \\
 \hline
 \text{Some X are not Z} & = & & = & \text{Some S are not P}
 \end{array}$$

One simple rule may be given for reducing any given mood to the form :

$$\begin{array}{c}
 \text{All M are P} \\
 \text{S are M} \\
 \hline
 \text{S are P}
 \end{array}$$

Take the conclusion of the given mood, as it stands, obverting it if it be negative :

See whether either premiss (or the converse equivalent) makes a predication concerning S, quantified as in the conclusion :

Take the predicate of such premiss (in its affirmative form), and see whether the other premiss predicates P of it universally. If, and only if, the given premisses conform to these conditions, the mood is valid. Thus :

$$\begin{array}{rcl}
 \text{Given the mood: (Fresison) No Z are M} & & \\
 \text{Some M are X} & & \\
 \hline
 \text{Some X are not Z} & &
 \end{array}$$

$$\text{We get : (conclusion) } \underbrace{\text{Some X are}}_{\text{S}} \underbrace{\text{non-Z}}_{\text{P}}$$

Treating this conclusion as a starting-point, we find the premisses are :

$$\begin{array}{l}
 \text{"Some M are X"} = \text{Some X are M} = \text{S are M} \\
 \text{"No Z are M"} = \text{No M are Z} = \text{All M are non-Z} = \text{All M are P}
 \end{array}$$

On the other hand, given (AAA, fig. 2) All Z are M  
 All X are M

---

All X are Z

We get (conclusion) All X are Z  
 S are P

(Minor premiss) "All X are M = S are M"

but since the other given premiss does not predicate Z of M universally, the necessary conditions are wanting.

---

## C

### THE UNIVERSALITY OF THE MAJOR PREMISS

[See p. 23]

THE chief direct use of the doctrine that the major premiss must be strictly universal is as against our tendency to rest our judgment about a particular case on a mere calculation of the probabilities. If there be one prize in a lottery of any number of tickets, my "certainty" that a given ticket will prove a blank belongs rather to practical than to theoretical wisdom. The fact that, for practical purposes, total neglect of unlikely chances is often our best course, does not make it *true* that unlikely chances are the same as non-existent ones. It is only through our ignorance of the causes, or through the hurry in which we are obliged to judge, that this course is defensible; and since neither our ignorance nor our hurry is constant in amount, no general rule can

be framed so as to take account of them. We have not even a standard by which to measure them at a given moment. It is true that attempts have been made to express numerically the point at which high probability becomes practical certainty,<sup>1</sup> but their failure is the most instructive fact about them. There are evident reasons why any such attempt must be too arbitrary to carry conviction.

Can we truly say that the force of an analogy lies in its approach to the type of direct generalisation? Yes, if we take care not to mean a *numerical* approach, like that in the case of the lottery-ticket. "All X *but one* are Y" is, in itself, equally unfit for the ground of an argument whether the heads of X are to be counted by tens, or billions, or any higher number. Before we can use it in a particular case, even for rough purposes, we need some glimpse of the causes involved, so as to judge whether that case belongs to the rule or to the exceptions. Nor does this necessity vanish when, as in drawing up insurance tables, the individual case is neglected in

<sup>1</sup> Dr. Venn (*Logic of Chance*, chap. xiv.) says, "The principal of these is perhaps Buffon. He has arrived at the estimate (*Arithmétique Morale*, § viii.), that this practical zero is equivalent to a chance of  $\frac{1}{100000}$ . The grounds for selecting this fraction are found in the fact that, according to the tables of mortality accessible to him, it represents the chance of a man of fifty-six dying in the course of the next day. But since no man under those circumstances takes the chance into the slightest consideration, it follows that it is practically estimated as having no value.

"It is obvious that this result is almost entirely arbitrary, and in fact his reasons cannot be regarded as anything more than a slender justification from experience for adopting a conveniently simple fraction; a justification, however, which would apparently have been equally available in the case of any other fractions lying within wide limits of the one selected."

favour of large groups of cases. Even then the principle is fully admitted that new circumstances render old statistics misleading,—which means that the statistics are not themselves the ground but only a handy practical guide to it, a cheap and convenient substitute for something better, namely, for a knowledge of the causes in operation. It is not the statistics which lead the insurance companies to divide us into our classes as premium payers, however much use may be made of statistics after the classes are formed.

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## D

### THE TYPICAL FORM OF SYLLOGISM

AT p. 63 it was said that under all forms of Syllogism the three parts, as there given, are distinguishable. Beginners may find a difficulty in reconciling this statement with the fact that in three out of the nineteen valid moods (viz. in *Disamis*, *Bokardo*, and *Dimaris*), the “major premiss” is a particular proposition. In these moods, and also in *Bramantip*, *Camenes*, and *Camestres*, it is only necessary to convert the conclusion—and so make major and minor premiss change places—in order to bring the syllogism under our scheme.

In taking the form in the text as typical, however, we need not forget that (as Lambert showed) the distinction of “Figures” has some justification apart from its mere mechanical possibility. No doubt the second figure



gives a natural formula—a formula less inelegant than *Celarent* or *Camenes*—for the proof of *difference* between X and Z, and the third figure gives a natural formula for the destruction of a general rule by means of a contradictory instance. We might state these two formulas as follows :

Fig. 2.—*Conclusion* : “X (or some X) differs from Z.”

*Premisses* : “They differ in regard to the quality M.”

Fig. 3.—*Conclusion* : “X does not safely indicate Z.”

*Premisses* : “Here is a case of X which is not Z.”

So far as our object is to classify useful and frequent forms of argument, it might be well to allow these two forms an honoured place in the scheme. But if our object be to state the most fundamental form only, there seems some reason to choose Fig. 1 for that purpose. The conclusion “S is P” represents a more finished, more assertive judgment than either (Fig. 2) “*X and Z are different*,” or (Fig. 3) “*Z cannot be inferred from X*.” These are rather stages on the way to judgment than deserving to be put on a level with the formula “S is P.” Both these “judgments” (if we call them so) are too indefinite to be used as premisses in a further syllogism. The assertion that *X and Z are different* is not equivalent to the assertion that “X is not Z” (or “No X are Z”), for the predication of non-Z is an assertion of *essential* difference between X and Z. There is always *some* difference between a Subject and its Predicate—or, in fact, between any two things or qualities in Nature,—so that to assert that S *is* P does not exclude the assertion that “S and P are different.” And similarly “*Z does not follow from X*” can never be a major premiss (*i.e.* an

inferential), and has to be altered into the more positive assertion, "There are Xs which are not Z" before it can become a minor. On this point see Venn's *Symbolic Logic*, chaps. vi. and vii., and Keynes' *Formal Logic*, Part II. chap. viii.

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## E

### ESSENTIAL RESEMBLANCE AND DIFFERENCE

[See p. 37]

IN saying that X will prove Y when and only when "essentially similar" cases are known to justify an "essentially similar" conclusion, we had better confess at once to stating the merest truism, one of those *dicta* so often met with in Logic, which carefully save their truth at the expense of their applicability to actual cases. The question whether a given resemblance is essential or only skin-deep is just as difficult as the question whether the ground of the argument is sufficient, being indeed only that same question in an altered form. If a conclusion be criticised,—if a critic thinks the ground insufficient,—his claim is that the assertion has confused essence with accident; and the assertor's mere re-assertion that he has not done so cannot be, by itself, convincing. Cases so nearly alike as to be distinguishable only by the clearest insight are not only capable of being essentially different, but are most of all able to mislead us effectively. The more permanent

any source of divided opinion is, — the greater the authorities in opposition,—the more certain it is that somewhere in the background lie analogies so plausible, cases of resemblance so high in degree, that the essential difference is exceedingly hard to discover or to bear in mind. High “degree” of resemblance is, in all the finer inquiries, the very thing to be most regarded with suspicion.

In cases of any great complexity, that is to say, in cases where opposition is serious and long-lived, our judgments as to the force of an analogy are so gradually built up as to render their examination a very difficult process. But though this consideration should lead us to distrust any short and easy method of settling ancient and recurring controversies, it does not prevent our gaining a real step towards such settlement by means of the notion that the force of analogy depends on the resemblance being essential<sup>1</sup> rather than otherwise striking — that is, visible rather to reason and sight together than to the naked eye. Nor does it prevent our seeing that in all such cases a difference of view as to the essentiality of some resemblance is the real source of the dispute.

The use of the word “essential,” therefore, in

<sup>1</sup> Students of Mill and Bain must not suppose that this statement conflicts with any doctrine that is taught by those great authorities. It conflicts *verbally* with the statement in Mill’s *System of Logic*, book iii. chap. xx. § 2, and Bain’s *Inductive Logic*, book iii. chap. xv. § 3 ; but this is because they are there seeking to define analogy (as well as it can be defined) in *contrast* with Induction. Any such definition could only have a value for rough purposes, and both Mill and Professor Bain would, I assume, have been quite ready to admit its inevitable defects.

regard to resemblance and difference, is not for settling disputed points offhand, but rather for drawing closer attention to the points that are actually doubtful. On mere difference and mere resemblance no inference can be founded, since any two things or events have points of resemblance and of difference to each other, and these may be as striking as possible and yet irrelevant to a given purpose. In speaking of the "essence" of anything, or in calling any resemblance or difference "essential," we are always stating an opinion of our own, at the risk of its being an error. And so the axiom that X will prove Y when and only when essentially similar cases are known to justify an essentially similar conclusion, is an expression of an ideal truth which is only more or less nearly applicable. Like any other axiom referring to the course of Nature, it will always be better applied by the good observer than by the bad one ; it therefore only states the aim of an inquiry, or points out what must be settled in order to reach a satisfactory result.

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